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ON THE FIXED POINTS OF A COLORIMETRIC SYSTEM

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ABSTRACT. The paper discusses the significance of the constants which enter into the specification of colour on the trichromatic system, and suggests certain fundamental considerations of a metrological character which ought to govern the choice of such constants in a standard reference system.

Various proposals which have been put forward from time to time are discussed in the light of these considerations, and the basis of the system which has been adopted at the National Physical Laboratory is explained.

Introductory

WHEN colorimetric measurements are expressed on the trichromatic system, the actual numerical values depend on the selection of the three "Primaries" of the system, and the "White" on which the quantity scales of the primaries are based, in accordance with the usual convention that numerically equal quantities of the primaries shall give a colour-match with the white. The term "white" does not, as is sometimes erroneously supposed, denote a single colour represented by a unique point on any colour triangle. Colour names have no such precise significance, but denote groups of colours which merge into one another in an indefinite manner. Thus red, orange, green, pink, etc., each serve to describe any colour which is better described by that name than by another. To specify a particular colour of any group it is necessary to mention some property or properties uniquely possessed by that colour and no other. The same applies to white, which is a name applicable to any colour which, seen by itself in the absence of other contrasting colours, does not exhibit an obvious hue. It will avoid confusion if the particular white which defines the units of any trichromatic system is referred to as the standard white of that system. It is not necessary, in the nature of things, that white should be used for

this purpose. As a matter of fact the "whites" used by the earlier investigators of colour problems have varied over a range of colour in excess of that within which the name can reasonably be applied.

In much of the theoretical work on colour problems which has appeared during the last fifteen years, the light given by a black-body furnace at a temperature of 5000° K. has functioned as the standard white. Such a source is not, of course, available as a practical illuminant, but its distribution of energy throughout the visible spectrum, being readily calculated from Planck's law, affords a convenient hypothetical standard for purposes of computation.

Alternative suggestions that have been put forward are that the standard white should correspond to "subjective white," or that it should be representative of "mean noon sunlight." The first of these suggestions is objectionable on two grounds. In the first place it involves defining subjective white on the basis of such limited information as has already been accumulated regarding it, and there is no justification for *defining* any psychophysical relation in this way. In the second place the adoption of this suggestion tends to associate a special significance with the standard white of a colorimetric system which does not necessarily belong to it, and all such unnecessary implications inevitably result in confusion of thought. The use of "mean noon sunlight" is a choice which at first sight appears a "natural" one, but ceases to be so when it is remembered that the only reliable data so far available apply to one place, Washington, and are not necessarily representative of places with different climatic conditions. In Britain, for example, mean noon sunlight is almost certainly appreciably yellower than that at Washington. Such a choice is, in fact, quite as arbitrary as any other that could be made. The suggestion, by purporting to give a special natural significance to the standard white, also shares the objection that it implies the importance of a quite unnecessary criterion of suitability.

The only theoretical requisite of the colour used for fixing the units of the primaries is that it shall require finite positive quantities of all three in order to match it. The only practical limitation on its choice is that it shall not be of such high saturation as to give inconvenient relative magnitudes to the primary scales. There is a large range of colour within which an arbitrary selection could be made without either loss or gain of convenience, and from the purely scientific point of view there would be considerable advantage in choosing for this purpose a light obtainable from some convenient unscreened laboratory source, such as a gas-filled tungsten lamp operated under standard conditions.

There is, however, a second purpose for which a white light is required in practical colorimetry. This is for the illumination of objects (either transparent or opaque) when measuring the colorimetric properties of such objects. A white light is not invariably used for this purpose, as in certain work the conditions necessitate the use of special illuminants of some other colour. For example, in the colorimetry of railway signal glasses, measurements with a white illuminant would be valueless, as the results would differ materially from those applicable to the use of illuminants like the oil flame or electric lamps used in the signal lanterns; but for a large amount of colorimetric work on materials no special illuminant is prescribed by the con-

ditions of use, and for such purposes it is desirable to prescribe some one standard illuminant. Here again there is no scientific reason why this illuminant should be white, the only scientific requirement being that its spectrum shall be continuous, and nowhere so deficient in energy, relative to other parts, as to mask the differences in colour of the light transmitted or reflected by illuminated specimens, and in this case also there would be many practical advantages, from the purely scientific point of view, in specifying an illuminant for general purposes of a character that is directly obtainable in the laboratory from an unscreened source.

Commercial requirements, real or supposed, make it necessary, however, that the standard illuminant shall give results for the measurements of coloured materials in accordance with their behaviour in sunlight, and for this reason a standard illuminant has to be chosen, sufficiently *near* sunlight in colour and energy distribution, for no two materials which give identical measurements with the standard illuminant to look dissimilar under average sunlight conditions. The standard illuminant must, therefore, be considerably whiter than the light directly obtainable from any laboratory source which has a continuous spectrum, and must involve the use of a blue filter for use in conjunction with such a source. The actual properties of the illuminant may, however, vary over a fairly wide range without introducing appreciable discrepancy between the relative behaviour of coloured materials, as measured, and their relative behaviour under ordinary sunlight conditions. Provided, therefore, that a standard illuminant is accurately reproducible and that its properties are accurately known, there are no severe requirements as to what these properties should be.

These two functions of a standard white light, one to fix the W point in the framework of the measuring system, the other to serve as an illuminant for material objects, are quite distinct, and could be fulfilled by different standards with a gain rather than a loss in the logical completeness of a standardised system of colorimetry. There is, however, considerable *practical* convenience in using the same standard for both purposes, as this reduces the number of independent standards which have to be maintained by the practical colorimetrist. For this reason a single standard illuminant has been adopted for both purposes in the system of colorimetric standardisation recommended by the National Physical Laboratory.

With regard to the primaries of a trichromatic system, considerable diversity of opinion exists as to the most suitable choice for general use in the representation of colorimetric relationships, and in particular for the relationships embodied in the trichromatic coefficients of the spectrum and the corresponding spectral distribution curves of the primaries.

The earlier investigators attached great importance to special sets of primaries, which they regarded as uniquely fitted to fulfil the requirements. Clerk Maxwell believed that three particular monochromatic radiations evoked primary colours corresponding with the primary sensations of the Young-Helmholtz theory, and thought that all colours could be produced by mixtures of these three stimuli in suitable proportions. The possibility of this conclusion being drawn was due to the inadequacy of Maxwell's experimental equipment, and subsequent investigators,

using better apparatus, found that there were no primary stimuli, whether monochromatic or otherwise, with which all the spectrum colours could be matched, and that in order to obtain colour matches the spectrum colours, in the regions between the primaries, had to be desaturated, either by the addition of white or of one of the primaries. Even after this was known, however, the tendency to give unique importance to certain spectral primaries persisted, and Abney (3) came to the conclusion that it was only with a green primary at a particular point in the spectrum that the intermediate spectral colours could be trichromatically matched even when diluted with white. It is idle to speculate on what purely instrumental difficulty can have led Abney to this erroneous conclusion, but the fact that he was led to it has been responsible for much confusion and misconception on the part of those who have approached the problem of colour vision through the medium of his work. It is, of course, immediately deducible from the colour triangle, and readily demonstrated in practice, that any three primaries, whether spectral or otherwise, with which it is possible to match white, can also be used to match any spectral radiation if the latter be diluted with white, or with one of the primaries. The relationships between such sets of primaries and the monochromatic constituents of the spectrum are identical in character, differing only in the numerical values of the various coefficients.

Spectral measurements, expressed in terms of any set of primaries consisting of real stimuli, are characterised by the existence of negative coefficients. By transforming the results so as to refer them to a set of primaries which lie sufficiently far outside the spectral locus on the colour chart for the locus to lie entirely inside the triangle defined by these primaries, the transformed values will have no negative coefficients. Just as there are an infinite number of sets of real stimulus primaries, there are an infinite number of sets of the other class, and a set may be chosen either at random or to suit some hypothesis or condition *external to the experimental data.* The primaries adopted by König are of this class, and were chosen to fulfil, as nearly as possible, conditions imposed by a certain interpretation of the Young-Helmholtz theory of vision. The primaries to which the König-Abney data were reduced by Weaver (4) are also of this class, though differing from those of König.

It has been customary to regard such primaries as being, in their nature, not stimuli but related to stimuli as effect to cause; and the trichromatic coefficients for any stimulus, when expressed in terms of these primaries, are regarded as representing the relative degree of excitation, produced by that stimulus, in three "elementary visual processes." These "elementary visual processes" are not necessarily identified with the three fundamental processes of the trichromatic theory of vision, but are related to them by transformation equations of mathematically identical character to those which correlate different sets of stimulus primaries. This distinction between stimulus mixture curves for real stimulus primaries and sensation mixture curves, or, more properly, visual process response curves, has been carefully drawn by modern as well as early workers, and the present writer has to plead guilty to perpetuating the verbal distinction in his own earlier papers. I did, however, point out in a recent publication (5) that the usual mode of regarding the ordinates of "sensation mixture" curves, such as König's, as indicating

the relative response of receptor processes to stimuli of different types and magnitudes was wrong, and showed that the only inferences which may properly be drawn from such curves relate to the *equivalence* of different stimuli in producing an identical, but unspecified, degree of response.

The real fact is that there is no difference whatever in the nature of the curves, whatever primaries are chosen. They are always stimulus mixture curves and can never be anything else. Exactly the same *facts*, as distinct from theories, are represented in all the different possible modes of representation, and these facts relate wholly to experimentally determined stimulus relations.

The choice of any set of primaries is merely the choice of a system of co-ordinates in which to express these relations, and the transformation equations for conversion from one set of primaries to another are simply the mathematical relations between the co-ordinates, with respect to different reference axes, of any given set of data. Neither the truth of the data, as really representing the properties of the eye, nor any property of vision, is involved in these transformations. The only properties of vision which enter into the problem are those fundamental properties which make a three-variable representation of the facts of colour-matching possible at all, and which enable us, in the first instance, to obtain a set of data in this form. After this stage we are no longer concerned with visual properties, as all processes of transformation from any one set of primaries to any other only affect the *numerical values* which express the same original data in different co-ordinate systems. The nature of the continuum in which we may regard the points representing the facts to be located is independent of the positions or directions of the particular co-ordinate axes from which we may choose to measure their positions.

Thus all points on the colour chart, whether inside the spectrum locus or outside it, represent stimulus values. Monochromatic stimuli lie on the locus. Compound stimuli, that is stimuli which require, for their physical definition, energy distribution curves which have ordinates differing from zero for at least two sensibly different wave-lengths, may lie anywhere on the chart. Those which lie inside the spectrum locus are "real," in the sense that they may have an objective existence in a particular sample of radiant flux. A stimulus represented by any point outside the locus is "unreal" in the sense that it cannot have a separate objective existence, as the ordinates at some part of its energy distribution curve must be negative, which is physically impossible; but suitable combinations of such stimuli, either with real stimuli or with other unreal stimuli, may have all-positive energy distribution curves, and so correspond to real stimuli. Mathematically, or dimensionally, there is no distinction between the two classes, and all trichromatic expressions of the colour-matching relations of the spectrum are essentially stimulus relations, whether the primary stimuli of the system are of the unreal class, as are those of Abney, König and Weaver, or of the real class. The conception of points outside the spectrum locus on the colour chart as stimuli may at first present some difficulty, but no greater intellectual effort is required to think in terms of hypothetical stimuli which have no existence except in combination than is required to conceive hypothetical fundamental sensations which have no existence except in combination. In both cases

we are simply analysing what *does* exist into convenient elements in order to express, in simple terms, the properties of such combinations of these elements as may have practical significance.

König's "Grundempfindungen" curves, or Weaver's "elementary excitation" curves are therefore merely the spectral distribution curves for hypothetical stimulus primaries. Before we can interpret them as sensation mixture curves, or as response curves of any part of the visual mechanism, it is necessary to introduce a *hypothesis* as to the relation between response and stimulus intensity. We must assume a one-to-one correspondence between stimulus and response. Not only so, but it must be the *same* one-to-one correspondence for all conditions of the eye for which the colour-match relations embodied in the curves hold.

This fact is clearly perceived by Judd, and is explicitly stated in a footnote to one of his recent papers (6), in which he defines the elementary visual processes, of which the Weaver excitation curves are to be regarded as the response functions, as "names for the activity, retinal or postretinal, which is in one-to-one correspondence with the incidence of radiant energy of a certain range of frequency upon the retina," and goes on to point out that the energy distribution curve of the stimulus with which this activity will be in one-to-one correspondence may contain negative ordinates.

It is clear, however, that immediately we *define* the activity of a visual process by any prescribed and fixed mathematical relation to a stimulus, it ceases to be an "activity" at all, being merely a new mathematical expression of the stimulus, and it is simpler to leave it out of account altogether, and confine ourselves to the use of stimulus terminology in dealing with the stimulus relations which alone form the logical basis of the curves.

I do not wish to suggest that an investigator who is *utilising* the data in the study of visual theory can serve no useful purpose by special choice of primaries, and by introducing special hypotheses as to the behaviour of the response functions of the eye. On the contrary, it is a perfectly legitimate use of the colorimetric data by the visual theorist, to put them in any mathematical form, and combine them with any hypothesis he may choose, in order to obtain confirmation or contradiction of such hypothesis; but this is a *use* of the data, and should not be confused with a *statement* of the data, which is a statement of the facts of colour-matching, obtained by experiment, and should be couched directly in terms which do not involve any of the hypotheses or theories which they may be used either to confirm or refute.

Thus any set of trichromatic distribution curves*, regarded as a statement of the facts embodied in them, must be described in stimulus terms, and the primaries, whether real or unreal, are of the nature of stimuli.

The fact that colorimetric data obtained in terms of one set of primaries can be converted to any other set by a mathematical transformation which does not alter the accuracy or nature of the original data, does not obviate the desirability of adopting

* The term "distribution curve" is suggested by Judd, in the paper already quoted, as a general substitute for the term "mixture curve" as customarily used. It seems from many points of view to be preferable to the older term, and less liable to ambiguous interpretation, provided it is used in a context which prevents confusion with energy distribution or luminosity distribution curves.

some standard set for the recording and comparison of colorimetric results. The transformation processes, although perfectly straightforward, are tedious when applied to a large number of data, and it is desirable, as a matter of convenience, that all results intended for publication either in scientific papers or in commercial specifications should first be converted to a common basis which renders them immediately comparable. That is to say, a standard reference framework is required as the basis of a standard system of colour measurement, regarded simply as a system of measurement, without reference to any of the various purposes, scientific or technical, for which the results of measurement may subsequently be used. The research worker on visual problems may find it necessary or convenient to convert the data to other bases for purposes in which he may be interested, and the technical investigator may also find that for some problems the results of his measurements may be more easily handled on some special system; but as a preliminary to all such *uses* of colorimetric data, the data itself should exist, in the first place, on a standard universally understood system of measurement.

This is generally agreed at the present time, but no such uniformity of practice has yet been adopted, except by the research workers in some of the industrial laboratories of this country, who use the system recommended by the National Physical Laboratory.

A trichromatic system is completely defined by specifying the three primaries and a standard white; in other words, by arbitrarily assigning the values, 1, 0, 0; 0, 1, 0; 0, 0, 1; and 1/3, 1/3, 1/3, on the three co-ordinate scales of the system, to four specified stimuli, exactly as we assign the arbitrary values 0 and 100 to two specified temperatures in order to obtain the centigrade temperature scale. We may assign these four fixed points on the colorimetric scales to any stimuli which it may be convenient to choose for the purpose, but we cannot arbitrarily fix more than these. With regard to the primaries it is felt by some that these should be of the unreal class. Two reasons are advanced for this preference. One of these is that "sensation" primaries, such as those of König, or the "elementary excitation" primaries of Weaver, are the "natural" elements in which to express the facts of colour mixture, because of the correspondence between the spectral distribution curves of such primaries and the response curves of the visual receptor processes. It has already been shown that such an interpretation is fallacious, and that no significance can properly be attached to any set of primaries which does not apply equally to any other. Even were this not so, such considerations belong to the field of visual theory, and are irrevelant to a decision on the most suitable "fixed points" of a colorimetric system, which should be chosen so as to give a straightforward expression of the results of measurement, and should not purport to convey anything else.

The second reason advanced in favour of "unreal" primaries is of a more practical character, and is that by their use we may avoid negative coefficients in all colorimetric equations. The writer has never felt, personally, that this advantage is of any importance. In many branches of experimental science the worker has to deal with negative values, both on instrument scales and on the standard metrological scales

on which the measurements are evaluated, and it is never suggested that such scales are practically inconvenient on this account. In using spectral distribution curves with negative regions, for evaluating colour from energy distribution data, a planimeter automatically attends to the subtraction of the negative portions.

It cannot be urged that the need to handle negative coefficients presents any difficulty to the scientific investigator, and as regards the technical colorimetrist, it appears reasonable to credit him with the same degree of competence in the use of his tools as is required by workers in any branch of applied science. In any case, any actual colorimeter *must* have real primaries, involving negative coefficients in the measurements of spectral or other highly saturated colours, as first obtained, so that anyone suffering from inability to understand or handle such coefficients simply cannot deal with colorimetric work at all.

Requirements of a Standard System

The selection of the primaries and standard white to be incorporated in a colorimetric reference framework ought, in the writer's view, to be decided solely from the considerations which govern the choice of suitable fixed points on metrological scales in general. These are:

1. The number of fixed points must exceed by one (and only one) the number of independent variables required to express the entity to be measured.

2. The particular samples of this entity to which the fixed points on the scales are arbitrarily assigned must be capable of simple physical definition in terms completely independent of the scales themselves or of any measurements implying their use.

3. The standard samples, in addition to fulfilling condition 2, must be susceptible of convenient and accurate reproduction, from their specifications, in the standardising laboratory, in order that the scales of actual measuring instruments may be calibrated against them.

I have not seen these conditions explicitly stated by any of the writers who have discussed standard reference systems; nor, in fact, do they appear to have been taken into consideration at all. Nevertheless they are as fundamental to a system of colour measurement as to any other system of measurement, and should take precedence over all other considerations with which they may be in conflict.

The standard white

Applying these principles to the present problem, the first essential characteristic of the four stimuli to be arbitrarily selected for the fixed points of the system is that they must be physically definable. Practical convenience necessitates that one of the four shall be such as to evoke a sensation of fairly low saturation, not necessarily white, but generally described as such and denoted by *W* on the colour chart. This stimulus, being necessarily compound, requires for its physical description an energy-distribution curve throughout the spectrum. Any hypothetical energy-distribution curve, such for example as that corresponding to a black-body at 5000° K., or the curve which is considered to give the best representation of average

noon sunlight, or other similar alternative, would fulfil condition 2. The third condition requires, however, that the *W* stimulus can be realised in the laboratory in a form which agrees with its specification to within the experimental errors of the best available practice. It is therefore necessary to employ the particular energy-distribution curve of a practicable source of light which can be used in the laboratory. Further, we cannot introduce the values of the energy-distribution curve into the specification, since it requires, in theory, an infinite number, and in practice a large number, of variables to describe such a curve, whereas there are only a few independently controllable variables available in constructing any actual illuminant. We must specify our white light as the stimulus provided by an illuminant constructed in accordance with a prescribed set of constructional data, all of which are mutually independent, and which are sufficient in number to give a unique character to the illumination. This procedure *specifies* rigorously some particular energy-distribution curve which may be approached in practice with an ever-increasing degree of closeness as laboratory technique improves.

The specification does not, however, *describe the properties* of the curve. These can only be known at any particular time to within the experimental tolerances determined by the accuracy of the processes available at that time for their measurement, and should be regarded as information concerning the standard, and not as part of its specification. It is important to bear in mind this distinction between the specification of a standard and a description of its properties. It is the properties of the standard in which the user is interested, and it may therefore seem that they should explicitly appear in the specification. This is only possible, however, if all the properties concerned are susceptible of independent adjustment when embodying the specification in a working standard. It is not possible in the present case, because to prepare an illuminant so as to have a specified energy-distribution curve would require the independent adjustment of the emissivity of a source (or the transmission of a filter) to specified relative values at from thirty to fifty different wave-lengths, which is entirely impracticable. This is the reason why, in the case of the N.P.L. standard white, the specification relates to the construction of an illuminant, and not to its properties. This illuminant consists of a gas-filled lamp used in conjunction with a liquid filter consisting of two solutions, A and B, contained in a double cell of clear colourless optical glass, each compartment of which is one centimetre in internal thickness.

Solution A

Cupric Sulphate	2·30 gm.
Ammonia (density 0·90)	230 c.c.
Distilled water to	1000 c.c.

Solution B

Cupric Sulphate	15 gm.
Cobalt Sulphate	15 gm.
Distilled water to	1000 c.c.

The specified procedure for rating the gas-filled lamp involves the use of an auxiliary filter of the same type as the "white light" filter, but with different concentrations of the constituents. The solutions for this filter are:

Solution A

Cupric Sulphate	1·41 gm.
Ammonia (density 0·90)	141 c.c.
Distilled water to	1000 c.c.

Solution B

Cupric Sulphate	11·0 gm.
Cobalt Sulphate	8·5 gm.
Distilled water to	1000 c.c.

This auxiliary filter is placed between a vacuum tungsten lamp, operated at a colour-temperature of 2360° K., and a Lummer-Brodhun photometer. The gas-filled lamp is placed on the other side of the photometer and adjusted in voltage until a colour match is obtained. In this way the gas-filled lamp is rated to a definite colour-temperature, although the actual value of this temperature does not appear in the specification. This procedure has been considered best owing to the uncertainties at present existing as to the temperature scale above 2500° K. The colour-temperature actually obtained in this way is at present believed to be very close to 2900° K.

The foregoing specification of the N.P.L. standard illuminant for colorimetry has been published, indirectly, in an Appendix to a recent paper by Wright(1), but no statement of its properties, nor the reasons for its adoption in its present form, have hitherto been published.

The use of these materials for producing a white-light filter appears to have been suggested by Prof. Pfund, and a filter of this kind was used, at his suggestion, by Sinden(2). No particulars of Pfund's filter were published, however, and when the matter became one of urgency in this country it was decided to work out such a filter *ab initio*, and also an auxiliary filter, using the same materials, for rating the gas-filled lamps.

The work of evolving these filters was carried out by the author's then colleague Dr A. F. A. Young, who finally arrived at the various concentrations given in the foregoing specification.

All numerical quantities in this specification are mutually independent. Each may be approached as closely in practice as available technique permits and mutual inconsistencies cannot arise however closely each element of the specification is realised.

There is only one weakness in the specification from the point of view of strict logical requirements. Gas-filled lamps vary slightly among themselves in the actual energy distribution corresponding to a given colour-temperature. The procedure laid down in the specification ensures that all lamps are rated to the same colour-temperature, but cannot take account of the slight differences in the energy values

at the middle of the spectrum which may occur when different lamps are so rated. The ambiguity arising from this cause is of no *practical* consequence, as it would be sub-limenal in any colorimetric relationship, and is very much less than the uncertainties which would arise in any attempt to work to a specified energy-distribution curve.

The light given by the N.P.L. standard illuminant is slightly yellower than average noon sunlight at Washington, but the degree of yellowness is not such as to introduce any discrepancy in the *relative* measurements of coloured materials and their behaviour in average sunlight conditions. On the other hand, the discrepancies between the relative properties of materials, as measured with such an illuminant, and their relative properties under ordinary artificial illumination are less than would be the case with a standard white of a bluer character. As this consideration is of some importance in connexion with many coloured fabrics used for dress or decorative purposes it suggests the practical desirability of keeping the standard white illuminant towards the yellow side of the practicable range.

The N.P.L. white has been in use for some years in several Industrial Research Laboratories, and has been found satisfactory, both as regards colour and constancy, for the purposes of these laboratories. The spectrophotometric transmission coefficients of the white light filter, as determined by Young, are given in Table I (p. 21), as are also the ordinates of the energy-distribution curve of the filtered light, assuming the colour-temperature of the gas-filled lamp to be 2900° K., and the ordinates of the luminosity curve, assuming the International standard visibility data.

The reference primaries

With regard to the stimuli to be chosen for the three other fixed points of the colorimetric scales, we may choose either monochromatic or compound stimuli. The former are completely specified by a statement of their wave-lengths.

Of the compound stimuli we must consider each of the two classes, real and unreal, in the light of the three conditions listed above. Compound stimuli, of either class, may be specified by energy-distribution curves, but for the same reasons already discussed in connexion with the white stimulus, primaries defined by an energy-distribution curve are unsuitable as the basis of a system of measurement. For this purpose the only logically accurate procedure would be to specify some particular illuminants which had energy-distribution curves approximating to the desired types. This would be possible in the case of real stimuli, but the practical inconvenience of working to such standards, when the simple alternative of adopting monochromatic primaries is available, is not counterbalanced by any advantage which would justify their selection as reference primaries. Thus of all possible real primaries, monochromatic stimuli are definitely the most suitable, both on account of simplicity of specification and ease of accurate reproduction in the laboratory.

I have already dealt with the usual arguments advanced in favour of a set of unreal primaries which would give freedom from negative coefficients at all parts of the colour field, and have given reasons for regarding these arguments as of no

importance. Judd (6) has, however, suggested a choice of unreal primaries based on rather different considerations, which merits attention, and which would, possibly, justify the use of such primaries in a reference system if this could be done without violating any of the three essential conditions I have mentioned.

On the colour chart corresponding to any set of primaries there is a straight line, the locus of all stimuli for which $\alpha L_R + \alpha' L_G + \alpha'' L_B = 0$, where α, α' and α'' are the trichromatic coefficients of the stimuli, and L_R, L_G and L_B are the luminosity factors of the primaries. All stimuli on this line have luminosity factors which are zero. The line has on this account been termed by Schrödinger the "Alychne" or lightless line. All stimuli on this line are unreal, as they necessarily have negative ordinates at some part of their energy-distribution curves. The line therefore lies entirely outside the spectrum locus. All stimuli whether real or unreal which lie on the same side of this line as the spectrum locus have positive luminosity factors. All those on the opposite side have negative luminosity factors.

Judd suggests that two of the reference primaries should be located on this line, the practical advantage of this choice being that on the system so obtained the luminosity factors of two of the primaries would be zero, which would effect appreciable reduction in the work of computing luminosity values from trichromatic data. The suggestion necessitates the use of at least two unreal primaries. Such primaries cannot be realised in practice and so cannot be directly embodied in laboratory standards as required by condition 3.

Provided, however, that we have the spectral-distribution data for any set of primaries, we can, apparently, get over this difficulty by using suitable monochromatic radiations as test stimuli for calibrating the working primaries of colorimeters, and obtain, from measurements on these, the necessary transformation equations for transferring directly from the instrument primaries to any other set whatever. The transformations were first given by the present writer (8), and would enable measurements to be converted to a system such as is suggested by Judd. At that time I considered the procedure would be of value for converting the results of measurements to a reference system with primaries which could not be directly embodied in laboratory standards. Further consideration showed, however, that such a procedure violates both conditions 1 and 2.

There are two ways in which we can define unreal primaries. We may define them by energy-distribution curves. In this case their physical specification is definite, but the transformation equations relating them to the standard monochromatic calibration stimuli can only be arrived at by calculation from a set of spectral-distribution curves. But the spectral values are not an integral part of the system of measurement; they are the *results* of measurement, and the assignment of numerical values to them presupposes that the four fixed points of the system are already defined on an independent basis. To use spectral data as a necessary connecting link between the real stimuli involved in experimental work and the reference primaries is, in effect, to define the reference framework in terms of the spectral locus, that is to say in terms of an infinite number of points which are not mutually independent.

This is inadmissible, and we are faced with the fact that we cannot obtain the transformations required to convert results from the form in which they are first obtained to any system of unreal primaries defined by energy-distribution curves, without *assuming* relations between the independent fixed points of the system.

There is, however, an alternative method of defining primaries which cannot themselves be realised as standard stimuli suitable for calibration measurements. We may specify them simply by their positions on the colour triangle of the system of which the standard calibration stimuli are the primaries. Regarding this process in the reverse direction it is equivalent to specifying the positions of the standard calibration stimuli on the system of the unreal primaries. If we give arbitrary constant values to these coefficients we obtain a system in which the fixed points are still determined by the standard calibration stimuli, but in which these are assigned arbitrary scale values other than 1, 0, 0; 0, 1, 0; 0, 0, 1. Such a system fulfils the three requirements of a logical reference framework equally with one in which the standard calibration stimuli are assigned the primary positions, and by suitably choosing the scale values to be assigned to them we may obtain a system with any primaries we may consider suitable.

Unfortunately this does not in itself enable us to adopt Judd's suggestion. In order to obtain zero luminosity factors for two of the primaries, these must lie exactly on the Alychne. The position of this line depends on the luminosity factors of the primaries. These factors are not essential properties of the fixed points of the reference system, but depend on the properties of the eye which makes the measurements. That is to say, they are an experimentally determined attribute of the system and not an integral part of it. To assign any set of luminosity factors to the primaries is, in effect, to incorporate in the reference framework a particular set of spectral-distribution curves. As previously stated, this is inadmissible, and there is no unique position of the alychne in the reference framework. If primaries were chosen to lie on the particular line corresponding to the best established data at any particular time, a new set would have to be chosen whenever fresh evidence made it desirable to revise these data, otherwise the primaries would no longer have the desired property. The position of the alychne cannot, of course, alter much with any feasible change in the luminosity data, but any alteration at all would involve small but finite luminosity factors for the two primaries concerned, and the computation problem would then be the same, as regards the amount of work involved, as if the factors were large.

It may be worth while to point out that the same saving in computation which could be obtained by making the luminosity factors of two of the primaries zero could also be obtained by choosing a set for which all three factors are equal. I have not worked out an actual case, but the principles involved are quite straightforward. A qualitative study of the possibilities suggests that it would not be possible to get a convenient set of primaries having this property in addition to that of giving all-positive coefficients for all colours, but if the unnecessary prejudice against negative coefficients be disregarded, and we open our choice to any region of the colour chart whether inside or outside the spectrum locus, it may be possible to obtain a set of

primaries having equal luminosity factors and at the same time giving convenient scales. From some points of view, such a choice might be preferable to that suggested by Judd, and an expression of the spectral data on such a basis might prove useful and suggestive to the visual theorist. As a basis for a reference system, however, it has the same defect as Judd's suggestion. The particular primaries which would have equal luminosity factors depend on the particular set of spectral-distribution data assumed.

We have to conclude, therefore, that in a standard reference system designed to fulfil the three essential conditions of a metrological system, we cannot base the choice of primaries on any arbitrary luminosity relations.

The reason why such considerations have either been overlooked or regarded as unimportant by modern writers is, I think, that the problem has always been considered in relation to the spectral-distribution curves, and, even then, only in relation to data intended to represent the normal eye. The object which predominates in most recent work is the definition of a normal eye for colorimetry, through the medium of a standard set of spectral-distribution curves, by means of which, in conjunction with spectrophotometric measurements on materials, the colorimetric properties of the latter may be computed, thereby obviating those uncertainties in direct colorimetric measurements which arise from the variations of colour vision of different observers. The problem is usually treated on the assumption that one set of spectral data suitable for this purpose is available, or is some day going to be available, and that the only question to be considered is how best to present these data. If the "normalisation" of colorimetric measurements and specifications for commercial and technical purposes were the only function of a standard system it would be justifiable, on grounds of expediency, to sacrifice logical consistency if by such means a system demonstrably more convenient in its practical applications could be obtained. It would be pure pedantry to object to any specially convenient system, consistent to within the limits which matter in practice, in favour of less convenient but strictly logical systems.

But commercial standardisation is not the whole, nor, from the scientific point of view, the most important function of a system of colorimetry. In the field of visual research there are numerous problems awaiting experimental investigation on which the available information is meagre and confusing because of the lack of any co-ordination in the bases of such measurements as have been made. The formulation of a scientific system of colour measurement, with scales based on the principles of metrology so as to have the same significance whoever may use them, would give an impetus to experimental research on the quantitative problems of colour vision, the need for which is amply shown by the fact that the meagre observations of a few pioneers have had to serve for so long as the basis for the discussion of all such problems. In work of this kind the properties of any agreed "normal" eye are irrelevant. The investigation may be concerned with the differences in the properties of different individuals, or with the correlation of all the properties of some one individual. If results of this kind are to be described in a common language, the reference framework must be independent of the properties of any

particular eye, or group of eyes, so that it may accommodate the observations of any individual whether normal or abnormal. For such purposes it is necessary to have a strictly logical system, with the necessary number of fixed points; and no others may be involved, either explicitly or implicitly, in the chain connecting the actual observations with the expression of the results. Specified luminosity factors, whether zero for two of the primaries or unity for all, cannot be incorporated in such a system.

This being so there is no advantage to be gained by choosing unreal primaries, and considerations of practical convenience and simplicity of specification suggest monochromatic radiations as the best choice for the primaries of a standard reference system.

The N.P.L. standard primaries

The question still remains which monochromatic radiations to choose. Each may be chosen from within a fairly wide range of the spectrum without materially affecting the convenience of the numerical scales of the resultant system. In selecting the primaries adopted at the National Physical Laboratory, it was considered that practical considerations ought to take precedence over all others. The fundamental purpose of a reference system is to express the results of measurements, and the utility of the system is determined by the facility with which measurements carried out in the laboratory can be accurately evaluated on the standard basis. Just as the simplest way of determining the constants of a mercury thermometer is to observe its readings at those temperatures, the ice point and the steam point, which are the fixed points of the mercury thermometer scale, the simplest method of determining the constants of any trichromatic colorimeter is to observe its readings on the four radiations which determine the fixed points of the standard colorimetric scale. The stimuli which determine these points should, therefore, be such as are conveniently obtainable for this purpose. By means of spectroscopic equipment, monochromatic light of any wave-length can, of course, be obtained, but the necessary equipment, and auxiliary calibration apparatus, required to obtain monochromatic radiation, of accurately known wave-length and free from impurity due to stray light, is only to be found in the larger optical laboratories. A much simpler method consists in making use of emission spectra containing only a few prominent monochromatic constituents, from which one suitable line may be isolated of sufficient purity by means of filters. The mercury arc provides two such monochromatic radiations, the green line at $\lambda = 0{\cdot}5461\,\mu$, and the blue line at $\lambda = 0{\cdot}4358\,\mu$, either of which can be obtained sensibly pure by means of suitable filters. There is no other equally convenient source with strong lines in suitable parts of the spectrum. A further advantage of the mercury arc is that such lamps are part of the usual equipment of the research laboratories of the colour industries, being employed, for example, for fading tests. By selecting these wave-lengths as two of the fixed points of the colorimetric reference system, the process of calibrating his colorimeter may be carried out by the technical colorimetrist without special and expensive apparatus.

The mercury arc does not provide a suitable red radiation for the third fixed point. A means of obtaining a red identical in colour with monochromatic radiation near the end of the spectrum is rendered possible by the fact that there is no appreciable hue change in the spectrum for wave-lengths beyond about $0{\cdot}67\,\mu$. Monochromatic light of any wave-length beyond this is colorimetrically identical. Filters are readily obtained which are sensibly opaque to all wave-lengths shorter than this. Such a filter, placed in front of any suitable electric lamp, provides a stimulus identical in colour with the terminal red of the spectrum, and can serve as a practical substitute for the monochromatic stimulus fixing the third point, if this is chosen of a wave-length within the range of constant colour.

Because of the great convenience of these three laboratory sub-standards for use in laboratories of limited equipment, the fixed points of the National Physical Laboratory system (other than the standard white) were chosen to correspond with the monochromatic radiations.

$$\text{Red, } \lambda = 0{\cdot}700\,\mu; \qquad \text{Green, } \lambda = 0{\cdot}5461\,\mu; \qquad \text{Blue, } \lambda = 0{\cdot}4358\,\mu.$$

As already explained, the choice of standard "fixed-point" stimuli does not necessarily involve constituting these stimuli the primaries of the system. By assigning arbitrary values other than 1, 0, 0; 0, 1, 0; 0, 0, 1, to these radiations, any other system of primaries is obtainable; but no advantage could be seen in this procedure to counterbalance the simplification which is obtained in the specification by making the "fixed-point" stimuli serve also as the primaries of the system, and this was accordingly done.

General convenience of the N.P.L. standard system

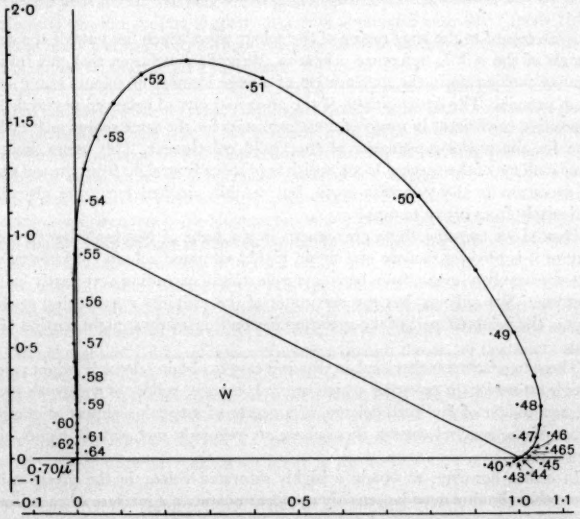
In the figure, the locus of the spectrum is shown on the colour chart of the N.P.L. system. The particular form of the chart shown is obtained by plotting the coefficients of the green and blue primaries, and by choosing a linear scale for the latter twice as great as for the former. This enables the essential characteristics of the locus to be better exhibited on a diagram of any given size than would otherwise be the case.

The spectral locus in this figure is based on the average of the trichromatic coefficients recently published by Wright (1), and the hitherto unpublished values obtained at the National Physical Laboratory. Details of the latter, and a comparison with Wright's values, are in the course of publication elsewhere (9). The figure is included here merely to facilitate reference to its qualitative features, and is not drawn with sufficient accuracy for quantitatively accurate values to be read from it.

Wright (7) has criticised the N.P.L. primaries on the grounds that they give a very unsymmetrical colour chart, a large part of the colour field being outside the triangle, with the consequent introduction of negative coefficients for a large proportion of colour measurements. He also points out that they are unsuitable for actual incorporation as working primaries in a colorimeter with spectral primaries, because of the large proportion of colours outside the working colour triangle, and

also because the blue primary is too near the end of the spectrum to get sufficient light of this wave-length from any convenient source.

The last objection concerning the blue primary is sound, and if it were either desirable or practicable to insist that all colorimeters should be of a type using spectral primaries, it would be worth while choosing standard reference primaries which could be conveniently used as the working primaries of such instruments. It is very undesirable, however, to choose a reference framework which places limitations on instrument design, or to assume that one set of working primaries is the most suitable for all purposes. In the writer's own colorimeter (8) the filters



normally fitted to the instrument are chosen to give the maximum convenience for the great majority of coloured materials with which the technical colorimetrist has to deal; that is to say, the great majority of these materials can be directly matched by positive quantities of the three working primaries, and it is only occasionally that the necessity of transferring one of the primaries to the test field arises in the colorimetry of materials. For example, all the fifty-seven samples in the *British Standard Schedule of Colours* for ready mixed paints issued by the British Engineering Standards Association, and the red and green glasses used at present for British railway signals, can be directly matched on this instrument with the primaries normally fitted. The yellow signal glasses require an almost negligible amount of minus blue.

It is only when dealing with highly selective dyes with narrow transmission bands in the green-blue region of the spectrum, or in the extreme red or violet regions, that primary transference is required to any appreciable extent. This involves little extra trouble but, inasmuch as it is extra, anyone engaged on work largely confined to such colours can increase the number of them which come inside the working colour triangle by replacing the usual green filter by another one of a bluer hue. This is mentioned as an example of the kind of freedom which should be left to the designer or experimenter in the choice of instrumental properties. It is therefore necessary to choose our "fixed-point" stimuli for convenience in calibrating any type of trichromatic colorimeter, rather than for convenience of direct incorporation in some one type.

With regard to the large region of the colour chart which lies outside the colour triangle of the N.P.L. reference primaries, Wright's conclusion that this involves negative coefficients in the specification of a large number of colours is not borne out in practice. The specifications of the great majority of coloured materials have all-positive coefficients in terms of these primaries for the same reason as they have them for the working primaries of the Guild colorimeter. This arises from the peculiar shape of the spectral locus, which is practically straight from the red end of the spectrum to the yellowish-green, but exhibits marked curvature elsewhere, particularly from $0{\cdot}50\mu$ to $0{\cdot}52\mu$.

Now if we consider these phenomena in the light of Newton's law of colour mixture it is obvious that we can obtain highly saturated colours, of hues varying from red to yellow-green, from heterogeneous stimuli extending over nearly half the spectrum. Not only so, but the maximum of the visibility curve being at about $0{\cdot}555\mu$, the brightest part of the spectrum lies on this nearly straight portion of the locus.

These two facts together explain why it is easy to obtain relatively bright colours of high saturation in materials which are red, orange, yellow or yellowish green; and, as a matter of fact such colours, of a degree of saturation almost as complete as that of the spectral colours themselves, are regularly met with in paints, dyed fabrics, etc.

In order, however, to obtain a highly saturated colour in the green-to-blue region, the stimulus must be sensibly monochromatic, as a mixture of constituents separated in the spectrum produces serious desaturation, owing to the pronounced curvature of the spectral locus. Sensibly monochromatic stimuli are never obtained by reflection from coloured materials, however strongly absorbent the material may be, because there is always a proportion of light reflected from the surface and from structural discontinuities close to the surface. Some fraction of the reflected light is thus inevitably spread over a large range of the spectrum. Even if this were not so, luminosity considerations would prevent the use of colouring materials which absorbed all but a narrow band of the spectrum. The narrowness of this band, and the fact that it is at a part of the spectrum where the luminosity is relatively low, would make the material so very dark as to be of little commercial use. In order to get even moderate luminosity for colours of such hues the stimulus must extend

over a large range of the spectrum, embracing practically the whole of the curved part of the locus, and the resultant colour is consequently of low saturation. This explains why we never find highly saturated colours of hues between green and blue among the materials of commerce.

Transparent materials, such as dyes in solution, coloured glasses, etc., being unaffected by reflections at or near the surface, may have colours of greater saturation, in this region, than is possible with reflecting materials; but, here again the impossibility of obtaining high saturation without great loss of light makes these of little practical importance. For example, in railway signal glasses, high saturation is very desirable and the red and yellow glasses give colours of sensibly 100 per cent. colorimetric saturation. The highest saturation obtainable with the "green" glass (which is really a blue-green) without reducing its transmission below a permissible limit is, however, only about 53 per cent.

Thus the region of the colour chart which is occupied by the great majority of coloured materials, while it extends right up to the spectrum locus from red to green, does not reach anywhere near it in the region from green to blue. This provides us with valuable guidance in choosing the most convenient working primaries for a colorimeter designed to give optimum convenience in general work. The green primary must lie on the nearly straight part of the spectrum locus. If it does not do so, colours will frequently be encountered which cannot be matched directly, and which require transference of the blue primary. By observing this consideration it is possible to obtain working primaries which will give a direct match of most colours, and with which the process of primary transference will rarely be called for.

This consideration, which is purely one of practical convenience, does not apply to the choice of standard reference primaries, but as a matter of interest it may be pointed out that the N.P.L. reference primaries, though chosen for the other reasons already discussed, fit in with this consideration very well. All the standard paint samples to which reference has already been made as giving all-positive measurements on the working primaries of the author's colorimeter, have also all-positive values when expressed on the N.P.L. reference system. Perry* has also pointed out how very rarely negative coefficients occur in the specification of a colour on the N.P.L. reference system.

It is true that the green primary could be moved appreciably in the direction of shorter wave-length without materially affecting this property, but not sufficiently far to make it worth while choosing a primary other than the standard calibration stimulus at 0·5461 μ.

Thus, although the non-occurrence of negative coefficients in technical colorimetry is not a vital consideration in the selection of a standard reference system, and, in the author's personal opinion, is not even an important secondary consideration, it does happen that the N.P.L. primaries, far from being an inconvenient set from this point of view, are much more satisfactory than would be the case with a green primary which materially reduced the blue-green region external to the colour-triangle. This region, though it looks large and important in the colour-chart, is

* Discussion on Wright's paper (7).

unimportant in the technical applications of colorimetry, as colours lying within it are relatively scarce.

In conclusion I should like to repeat the view which I expressed in the discussion of one of Wright's papers, that problems connected with the choice of a standard reference system for colorimetry ought to be treated, as I have endeavoured to treat them in this paper, free from all sentimental considerations. I do not use "sentimental" in any contemptuous sense, but as referring to preconceived feelings about things, and ways of looking at things, based upon the points of view and methods of approach of earlier workers, which are difficult to avoid. When I first approached the task of evolving a standard basis for colorimetry at the National Physical Laboratory, I was strongly endowed with the same sentimental prejudices, and regarded as highly desirable that a standard system should have primaries, and also a standard white, to which some unique psycho-physiological significance would attach, and that the system should be what many still seek, a *natural* framework for expressing colorimetric results. It was only the practical experience obtained in trying to obtain a system which would fulfil the requirements not only of the visual theorist but of the practical colorimetrist, whether engaged in visual research or in technical colorimetry, that forced the abandonment of these prejudices, and showed that the problem could only be treated as a straightforward problem of metrology, analogous in every essential respect with other such problems.

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Table I. *N.P.L. standard white light*

λ = wave-length (in μ).
V = ordinate of International Visibility Curve (I.C.I., 1924).
t = transmission coefficient of white light filter.
E = ordinate of Energy Distribution Curve, assuming source at colour-temperature of 2900° K.; values reduced so that $E = 100$ at $\lambda = 0{\cdot}56\mu$.
VE = ordinate of luminosity curve.

NOTE. Values of E and VE at every $0{\cdot}005\mu$ obtained by interpolation.

λ	V	t	E	VE	λ	V	t	E	VE
0·38	—	—	28·0	—	0·575	—	—	99·3	90·31
0·385	—	—	32·0	—	0·58	0·870	28·7	97·9	85·15
0·390	—	—	35·5	—	0·585	—	—	$96{\cdot}1_5$	79·29
0·395	—	—	39·0	—	0·59	0·757	26·2	94·7	71·66
0·40	0·0004	92·1	43·2	0·017	0·595	—	—	93·6	64·80
0·405	—	—	47·4	0·036	0·60	0·631	24·1	$92{\cdot}1_5$	58·15
0·41	0·0012	92·0	$51{\cdot}5_5$	0·062	0·605	—	—	90·6	51·26
0·415	—	—	55·7	0·120	0·61	0·503	22·2	$89{\cdot}4_5$	45·00
0·42	0·0040	90·8	$60{\cdot}1_5$	0·241	0·615	—	—	88·7	39·28
0·425	—	—	64·0	0·473	0·62	0·381	20·8	$88{\cdot}0_5$	33·57
0·43	0·0116	86·8	67·3	0·781	0·625	—	—	87·6	28·09
0·435	—	—	70·1	1·161	0·63	0·265	19·7	87·4	23·16
0·44	0·023	80·8	72·6	1·67	0·635	—	—	87·4	18·95
0·445	—	—	74·4	$2{\cdot}27_5$	0·64	0·175	18·9	$87{\cdot}6_5$	15·34
0·45	0·038	73·6	75·9	$2{\cdot}88_5$	0·645	—	—	$87{\cdot}8_5$	12·20
0·455	—	—	77·1	$3{\cdot}72_5$	0·65	0·107	18·2	$87{\cdot}9_5$	9·41
0·46	0·060	66·7	78·2	4·69	0·655	—	—	$87{\cdot}9_5$	7·24
0·465	—	—	79·2	5·87	0·66	0·061	17·5	87·9	5·36
0·47	0·091	60·5	80·1	7·29	0·665	—	—	87·7	3·85
0·475	—	—	80·8	9·13	0·67	0·032	16·8	87·5	2·80
0·48	0·139	54·7	81·2	11·29	0·675	—	—	$87{\cdot}2_5$	2·03
0·485	—	—	81·5	13·91	0·68	0·017	16·1	$86{\cdot}9_5$	1·48
0·49	0·208	49·4	$81{\cdot}6_5$	16·98	0·685	—	—	$86{\cdot}6_5$	1·04
0·495	—	—	$81{\cdot}5_5$	20·96	0·69	0·0082	15·5	86·3	0·71
0·50	0·323	44·2	80·9	26·12	0·695	—	—	86·0	0·503
0·505	—	—	$79{\cdot}7_5$	32·40	0·70	0·0041	14·9	$85{\cdot}6_5$	0·351
0·51	0·503	39·3	79·1	39·77	0·705	—	—	85·2	0·251
0·515	—	—	79·4	48·15	0·71	0·0021	14·3	84·6	0·178
0·52	0·710	36·9	81·1	57·57	0·715	—	—	$83{\cdot}9_5$	0·121
0·525	—	—	83·9	66·47	0·72	0·00105	13·7	83·2	0·087
0·53	0·862	36·3	$86{\cdot}7_5$	74·77	0·725	—	—	$82{\cdot}6_5$	$0{\cdot}062_5$
0·535	—	—	89·6	81·74	0·73	0·00052	13·1	81·6	$0{\cdot}042_5$
0·54	0·954	35·7	$92{\cdot}3_5$	88·09	0·735	—	—	80·7	0·029
0·545	—	—	94·8	93·15	0·74	0·00025	12·5	79·7	0·020
0·55	0·995	34·8	97·1	96·62	0·745	—	—	78·9	0·013
0·555	—	—	98·9	98·81	0·75	0·00012	11·9	77·7	0·009
0·56	0·995	33·4	100·0	99·50	0·755	—	—	76·4	$0{\cdot}006_5$
0·565	—	—	100·3	98·39	0·76	0·00006	11·3	75·1	$0{\cdot}004_5$
0·57	0·952	31·3	100·1	95·30	0·765	—	—	73·9	—
					0·77	—	10·7	72·7	—
					0·775	—	—	71·3	—
					0·78	—	10·1	69·8	—

DISCUSSION

W. D. Wright: Progress in colorimetry is at present dependent on two things; first, on the design of suitable apparatus with which to make accurate colour measurements, and secondly, on the existence of a standard reference system. The instrumental difficulties are being overcome and in fact for certain classes of work, apparatus is already available; but standardisation at present lags behind. This paper of Mr Guild's, by discussing some of the more pertinent features of the problem, should materially assist in obtaining an agreed standard system.

The N.P.L. standard white has proved to be easy to prepare and convenient to use in practice and its colour can safely be regarded as a good average white. I do not see that there should be any objection to its acceptance as a standard white for colorimetry.

The suggested method of specification is, I think, more open to question. I should prefer a white defined by its energy distribution curve although, as Mr Guild points out, it is at present impossible to manufacture lamps with sufficient accuracy to reproduce a given energy curve. I can, however, see no great objection to specifying, say, two limiting curves within which the actual energy curve must lie; for computation purposes the mean curve could be used. Alternatively, the mean curve could be specified and tolerances stipulated at various wave-lengths. The method of producing the standard would remain the same and the tolerances would exist in both cases. In one instance, however, they are definitely stated, whilst in the other they are ignored. In both cases the energy curve is required for theoretical work and the energy curve method would seem more fundamental than the chemical formula.

I have commented elsewhere on the choice of the N.P.L. reference primaries and in spite of the defence put up by Mr Guild in this paper I still feel that from several points of view an all-positive set of primaries is to be preferred. Mr Guild has given certain principles that should govern the choice of a rigorously scientific set of reference standards; but as he points out, it is not really necessary to enforce these conditions rigidly for commercial purposes. The choice need not, therefore, be so limited as he suggests.

For reasons given in greater detail in my last paper and referred to by Mr Guild, I believe commercial colorimetry will develop mainly along spectrophotometric lines. That being so, no reference to standard primaries would be involved, except that indirectly implied in the trichromatic distribution curves, the conversion to trichromatic units requiring merely the measurement of the areas under three curves. This measurement, whether carried out by planimeter, mechanical apparatus involving specially shaped cams or by actual addition on squared paper, is definitely simpler if the areas under the curves are all-positive. Further, I believe that for the non-technical person, many of whom would be intimately involved in the further commercial development of colorimetry, it would be easier to grasp the nature of the problem if all negative quantities could be avoided. Although to do this would

necessitate the introduction of primaries that could not be reproduced physically, I think the net gain would rest with the positive primaries. Lastly although, as Mr Guild mentions, negative quantities cause no confusion in such matters as temperature measurements, I cannot recall any parallel in the commercial world in which some article is ordered by a negative number. That would sometimes happen were the N.P.L. primaries to be adopted for commercial standards.

I think the above arguments would justify any firms who were using spectrophotometric methods of colour measurement, in advocating the adoption of all-positive primaries. Those using other methods involving, say, three-colour matching might be indifferent or might reasonably prefer real primaries; whilst research workers interested in referring their results to as definite and independent a set of co-ordinates as possible and those interested in co-ordinating data already in existence could make out a strong case for adopting monochromatic primaries.

The compromise that will be necessary is obscured at present by the existing uncertainty of the final direction in which colour-measuring technique will develop. The most desirable feature at the moment is that progress should actually be encouraged and no longer hindered by the lack of reference standards, and from this point of view it may well be that since Mr Guild has a complete reference system worked out and one which, moreover, has had the advantage of already being tested commercially, its adoption might so aid colorimetric progress as to offset the disadvantages I have mentioned.

Mr Irwin G. Priest (communicated): Mr Guild surely deserves to be most heartily congratulated on having given a remarkably lucid and cogent exposition of the most fundamental principles of colorimetrics. I would commend and indorse particularly his clear treatment of the confused and vexed question of what is "white light." The "white-light" problem in colorimetrics is largely a linguistic one which has arisen because our concepts and technique have outgrown our terminology, and we have fallen into the confusion necessarily resulting from calling several distinct but closely related things by one and the same name. To show how closely Mr Guild and I are in agreement on the fundamental principles which may guide us in resolving this verbal difficulty, I venture to quote from a discussion which I wrote more than a year ago but which has not yet been published, although the substance of it was presented orally at the Thomas Young Meeting of the Optical Society of America, October 26, 1929*.

"It is first necessary to recognise clearly the vagueness of certain terms in current use, and attempt to formulate some more precise ideas. In increasing order of vagueness of meaning these terms are 'sunlight,' 'daylight,' 'white light.'

"The spectral distribution and colour of direct sunlight at the earth's surface depend upon: (1) variability in the emission of the sun itself; (2) variability in the transmission of the earth's atmosphere; (3) the altitude of the station where the

* The quotation is from my discussion of the British Committee's proposal to establish a standard of "white light" (I.C.I. document "15 Great Britain 1," received in America June 25, 1929, and published in the programme of the Thomas Young Memorial Meeting of the Optical Society of America, Supplement to October, 1929, issue of the *Journal of the Optical Society of America*).

sunlight is received; (4) the zenith distance of the sun, which depends upon the latitude of the station, the declination of the sun (time of year), and the hour of day. The extremes of variation are from a colour temperature lower than that of a candle flame (sunrise or sunset) to a colour temperature greater than 5400° K. (noon at the summer solstice). By successive specifications we may make more precise what is meant by 'sunlight.' Typical examples of increasingly elaborate specifications to give greater precision to what is meant are: 'sunlight,' 'noon sunlight,' 'noon sunlight at sea level,' 'noon sunlight at sea level in latitude 40°,' 'noon sunlight at sea level in latitude 40° at the summer solstice,' 'mean noon sunlight at sea level in latitude 40° at the summer solstice,' etc.

"The colour of the various phases of 'daylight' covers a still wider range. By daylight one may conceivably mean anything from direct sunlight with the sun near the horizon (about 1700° K. or less) to blue sky light (over 20,000° K.). The latter extreme ('north skylight') has indeed the prestige of having been regarded and used as a standard by dyers and colourists for many years past.

"Current meanings of the term 'white light' are exceedingly diverse and vague, as indicated by the following illustrations:

"(1) To many physicists this term suggests nothing more than radiant energy having a continuous distribution in the spectrum, without any regard to the precise specification of that distribution.... In this sense a 'black body' emits 'white light' at any temperature; and candle light, sunlight and skylight are all 'white light.'

"(2) The term 'white light' is often used as a synonym for 'daylight,' involving all of the vagueness inherent in that term.

"(3) In the *Standard Dictionary*, 'white light' is defined as 'the undecomposed sunlight.'

"(4) 'White light' has a distinctly psychological significance; namely, the neutral (hueless) stimulus, which can be determined only by psychophysical experiment*.

"(5) By arbitrary convention in colorimetrics, the centre of the Maxwell triangle (the point having the co-ordinates $r = g = b = 1/3$) is sometimes designated as the 'white point' indicating that it represents 'white light.'

"It is obvious that we cannot use the term 'white light' in these several senses in the same discussion without introducing great confusion. In colorimetry, this is an actual practical difficulty—not a mere matter of pedantry. For example, we may wish to discuss the colour of daylight relative to 'white light' understood as under (4) just above; evidently we cannot then use 'white light' as a synonym for 'daylight.' Or we may have constructed a Maxwell triangle with the centre representing 'sunlight'; if, then, we say that the centre represents 'white' we introduce great confusion when we essay to place in this triangle the points representing several other 'white lights' which we have independently defined (either explicitly or tacitly) as 'white light.'

"It therefore appears necessary that we introduce some preliminary discussion for the purposes of clarifying and fixing our ideas, developing an adequate and

* The actual significance of the term, in this sense, is still very vague.

suitable terminology, and envisaging as distinctly as possible the nature and purpose of the standard which we desire to establish.

"From the practical point of view, one of the chief purposes of colorimetry is to specify the so-called colours of non-luminous objects and materials when they are illuminated. The objects, of themselves, have no colours. The colours only appear when the objects are illuminated, and these colours are dependent upon the illuminant. In establishing a system of colorimetry it is therefore necessary to specify the source of light which is to be standard for that system. Radiant energy is the adequate stimulus of colour. The necessary and adequate specification of the illuminant is made in terms of spectral distribution of energy. I propose to call the energy which is emitted by the standard source the *prime stimulus* and to specify it by its spectral distribution.

"With regard to what would be desirable (considering the practical purposes which our system of colorimetrics is to serve), we may select an ideal prime stimulus which we cannot realise exactly in laboratory practice. However (as will be seen below), we can set up a working standard. In practice (as exemplified by some so-called 'artificial daylight lamps') we set up a lamp and filter devised so as to deliver radiant energy competent to evoke accurately the same colour as would be evoked by the ideal prime stimulus and having as nearly as possible the same spectral distribution. We agree to use this device to illuminate objects whose colours we compare and objects whose colorimetric specification we determine by observation. The energy so provided is the *working-standard prime stimulus*, and is to be specified by its own spectral distribution, which, in general, will differ slightly from the spectral distribution of the ideal prime stimulus.

"In establishing a complete system of colorimetrics, it is also necessary to select and specify still another standard, which we shall now discuss. For the purpose of computing trilinear co-ordinates, dominant wave-length, and purity from data on the spectral distribution of colour stimuli, it is necessary to assign a unique position to some stimulus which is to be represented by the centre of the Maxwell triangle. The spectral distribution of this stimulus becomes the basis of the computation of the co-ordinates which are to represent any other stimulus, so that all computed values of trilinear co-ordinates, dominant wave-length, and purity are dependent upon this standard. Because it does constitute such a *basis*, I propose to call it the *basic stimulus*. . . .

"It will now be instructive to consider the relations between the prime stimulus and the basic stimulus. Let us start by setting their definitions side by side for comparison.

"*The prime stimulus is the radiant energy incident on the sample so that the modification of it by the sample gives rise to the colour which is to be specified.*

"*The basic stimulus is a selected stimulus which (by arbitrary convention) is represented by the trilinear co-ordinates* $r = g = b = 1/3$ *in a given system of colorimetrics.*

"We may now consider their characteristics and inter-relations as follows:

"(1) The prime stimulus and the basic stimulus are both conceived of as heterogeneous radiant energy.

"(2) They may both be adequately specified in terms of spectral distribution of energy....

"(3) The selection of a standard for either the prime stimulus or the basic stimulus may be said to be arbitrary in the sense that different choices may be made to accord with the diverse purposes of special problems in colour specification; but it may not be considered arbitrary in the sense of being a matter of indifference after the purposes of the colour specification have been fixed.

"(4) The standard for the prime stimulus should naturally be representative of the illumination under which the colours of the objects in question will be a matter of interest.

"(5) Aside from specific considerations which may outweigh it in special cases, as noted below, the most natural and logical choice of a standard for the basic stimulus in general colorimetrics might be the neutral stimulus*.

"(6) In some problems the choice of a basic stimulus may not be identical with the choice of a prime stimulus. A practical example of current importance is the specification of the colours of 'signal glasses.' Here, undoubtedly, the standard prime stimulus should be either identical with the energy emitted by the source used (e.g., the kerosene flame or the electric filament lamp) in conjunction with the filter to produce the signal in actual practice, or at least, should be fairly representative of it. On the other hand, there is some question as to what would be the most suitable choice of a standard basic stimulus on which to construct a system of co-ordinates for representing the colours of the various signals. If the signal system in question is for strictly nocturnal use, there would perhaps be reason for making the basic stimulus representative of the electric incandescent lamp. For a daytime system, we might, for like reason, choose the basic stimulus to represent daylight, although it would certainly not be permissible to choose daylight for the prime stimulus. In both cases we might conceivably argue that the neutral stimulus should be adopted as the basic stimulus†.

"(7) In by far the greater number of colour specifications (in particular the specification of the colours of objects seen by the light which they reflect), the interests of convenience and simplicity will be served by making the standard basic stimulus identical with the ideal prime stimulus. Any other choice would introduce needless complexity in the computation of trilinear co-ordinates from data on spectral reflectance as well as very undesirable confusion in interpreting the results of such computation. It is a well-known and significant fact that non-selective diffusely reflecting surfaces are constantly *perceived* as 'white' (or 'grey') for a wide range of variation in the spectral distribution of the illuminant. Whatever may be our choice of prime stimulus we wish to make the trilinear co-ordinates of such a non-selective sample express this fact by making them be $r = g = b = 1/3$. This is accomplished by making the basic stimulus identical with the prime stimulus. Accordingly we may contemplate the establishment of a general system of colorimetric computation and specification in which the standard basic stimulus is identical with the ideal

* Provided its spectral distribution were determined with a sufficient approximation to finality; but it cannot be said to be so determined.

† Personally, I would not so argue.

prime stimulus. The stimulus which is adopted in this double rôle may be called the ideal *prime-basic stimulus*."

I believe the close agreement of ideas and principles enunciated independently by Mr Guild and myself in regard to the idea of "white light" and its function in colorimetrics will be obvious; and I hope that this augurs well for the prospect of obtaining international accord on the fundamental standards of colorimetrics.

It will be noted that the only outstanding difference between Mr Guild's discussion of the meaning of "white light" and my own is that I have put myself to the trouble of inventing three new terms (*prime stimulus*, *basic stimulus* and *prime-basic stimulus*) in order to circumvent the intolerable confusion incident to the use of the term "white light" in several distinct senses in the same discussion. I believe such distinct names will serve a useful purpose in helping to *keep* Mr Guild's very clear and distinct concepts clear and distinct in the minds of others. These names are now in current use in our local conversation at the Bureau of Standards; and we find them useful in helping us to say clearly what we intend to say, and in helping us to avoid unnecessary circumlocution and prolixity*.

I am not disposed to insist on the merit and propriety of these particular terms if someone can suggest better *names* for the same *things*; but I do feel very strongly that *some* distinctive suitable names are much needed to designate respectively the several distinctly different things which have all been called "white light" quite indiscriminately and vicariously for many years.

Mr Guild's emphasis on the concept that colours are to be specified in terms of physical stimuli is also to be commended. It is well worthy of emphasis that the fundamental *specification* of colours (as contrasted with the mere qualitative vague *description* in psychological terms) consists merely in specifying in some manner or other the physical stimuli which are correlated with the colour† under certain conditions and circumstances.

I agree entirely and emphatically with Mr Guild in the opinion that "subjective white" has no title whatever to be considered as a candidate for the position of standard basic stimulus *for the purposes of specifying the colours of materials*‡.

I do not agree with Mr Guild in his attaching such slight importance to natural criteria of suitability in the selection of a standard prime stimulus§. From the point

* These terms are used in a forthcoming publication by my associate Dr Deane B. Judd, "Chromatic sensibility to stimulus differences."

† Colour itself is a psychological entity and cannot be conceived as consisting of elements of the physical universe.

‡ I conceive that the idea of the neutral or hueless stimulus (Mr Guild's "subjective white") may be of future interest in the theory of the visual response and that it may be desirable sometime to construct a Maxwell triangle with it as the basic stimulus, for purposes entirely distinct and separate from the business of practical colour specification. However, a great deal more research remains to be done before we can attach any great significance to data on the hueless stimulus, for any purpose. It is now a very vague concept entirely devoid of interest from the point of view of practical colour specification. I plan to review and revise, by other experimental methods, my own data on the hueless stimulus (*B. S. Sci.* Pap. 417), and, in the meantime, I attach but very little significance to my preliminary data.

§ Paragraphs 5 and 6 of the paper (p. 2).

of view of *metrology for metrology's sake*, Mr Guild is undoubtedly quite correct in his contentions. But I hold that, in this connexion, metrology is not to be regarded as an end in itself but as a means to an end. It seems to me to be an obvious truism that our purpose in general colour specifications should be to set up a system of metrology which will serve to specify colours of materials in a manner as nearly congruent as possible with the relative colours of the materials *under the most prevalent illumination which is to be effective in viewing the materials*. In selecting a standard prime stimulus, we are, in effect, merely selecting what may be accepted as the most suitable compromise illuminant* to be used in comparing the colours of various samples. The practical colourist, from time immemorial, has insisted on comparing samples under "north skylight" and he will probably continue so to compare them unless he can be won to the reasonable idea of accepting "average daylight" as a standard prime stimulus. I consider it unreasonable to request him to accept sunlight. The practical colourists stand out for their traditional standard, north skylight. The colorimetric experts of the physical school adhere, by their tradition, to direct sunlight (at noon). "Sunlight" is *not* a suitable representative of "average daylight"; "north skylight" is *not* a suitable representative of "average daylight." It appears that "average daylight" (overcast sky) would be a most happy compromise standard from a diplomatic point of view.

I hold that the most suitable choice for a prime-basic stimulus, in all cases *where there is no specific reason for another choice*, is "average daylight"; and, on the basis of a mass of data now available, I would accept the radiant energy from a Planckian radiator at 6500° K. as a representative of "average daylight" suitable to all countries†.

At the same time, I would remark that, in order to meet all of the needs of colour specifications, it will no doubt be necessary to adopt eventually not merely *one*, but *several* suitable standards for the *prime-basic* stimulus in systems of colorimetrics designed for specific purposes. I have emphasised "average daylight" merely because it is the pre-eminent natural candidate if our purpose is to specify the colours of objects viewed by daylight, and because it may as well be accepted as an arbitrary standard *in all cases in which some other choice is not required for specific reasons*. If our purpose should be to specify colours of objects viewed as illuminated by the gas-filled tungsten lamp, the natural and suitable choice for a prime-basic stimulus would be the energy from such a lamp, which, for simplicity and convenience, might be specified by the spectral distribution of energy for a Planckian radiator at about 2800° or 2900° K. The perfectly general criterion for choice is that, for the purposes of computing trilinear co-ordinates to be used in specifying the colours of objects, the prime-basic stimulus should be specified by the spectral distribution of energy which represents, to a sufficient approximation, the spectral distribution of energy for the illuminant which is to be used in viewing the objects. In general, we cannot usefully specify the colours of objects, as viewed in daylight,

* With regard to "quality of colour" or, more specifically, spectral distribution of energy.

† I hope to justify this position fully in the discussion which I am preparing for the I.C.I. Committee on Colorimetry.

by trilinear co-ordinates computed on the basis of a prime-basic stimulus specified by the spectral distribution of a Planckian radiator at some relatively low temperature such as 2800° or 2900° K.

In view of the actual situation in the United States, I must take exception to Mr Guild's statement: "...no such uniformity of practice has been adopted, except by the research workers in some of the industrial laboratories of this country, who use the system recommended by the National Physical Laboratory." The system based on the "O.S.A. Excitations" and "Washington Mean Noon Sun"* is quite well established in America and is used by the Bureau of Standards and others in the practical specification of colours. It is rather firmly embedded in American practice. However, I hasten to add that I have no desire to assist in perpetuating this system for general purposes after some other system may have been proven notably more consistent with experimental results of stimulus mixture for the "average observer." As indicated above, I do not now regard "Washington Mean Noon Sun" (or the so-called "Abbot-Priest Sun") as the most suitable standard for a prime stimulus in general colour specifications. In my opinion, the "black-body" at 5000° K. (or 4800° K.) is a still less suitable standard. Both of these standards were originally adopted as the result of the error committed in accepting them as suitable representatives of "average daylight" in colour specifications. From this point of view, they are both obsolete, and are continued in use merely to avoid the inconvenience of making a change. For certain purposes (notably, in our standardisation of Lovibond glasses for the vegetable-oil industry), it will probably be necessary to adhere to our present system (O.S.A. Excitations and Abbot-Priest Sun) indefinitely in the future, for the sake of continuity with a vast amount of past computation. On the other hand, I am firmly convinced that we should look forward to establishing a more suitable system for more general purposes in due season.

Mr J. Guild: I have to thank Mr Priest for an important contribution to the discussion on my paper. These matters are now of such wide international interest that discussion confined to workers of the country in which any particular paper appears is of necessity limited in value as compared with discussion on a broader geographical basis where the exigencies of time and language make this possible. Those who have followed my own various papers on colorimetric matters will realise the extent to which the development of this science in Great Britain, and, in particular, at the National Physical Laboratory, has been based on the inspiration of the earlier modern American work, which, from our point of view, began with the work of H. E. Ives and others some fifteen years ago. Such differences of outlook and method as may have arisen between ourselves and our colleagues in the United States in the later developments of the subject are, as Mr Priest points out, and as his communication shows, not differences in fundamental principle. In my own view they are merely such differences of outlook as inevitably arise when different investigators approach the same group of problems in a somewhat different

* Especially the spectral distribution of energy known as "Abbot-Priest Sun," which is merely one particular representation of "Washington Mean Noon Sun."

order, which results in differences in the relative weights which they are led to attach to various practical considerations. As some of these considerations are contradictory, some have to be sacrificed to others, and I think the differences in present outlook in this country and the United States are mainly on questions of which practical considerations should take priority.

It is hardly necessary for me to emphasise the important and fundamental points on which Mr Priest is in agreement with the views put forward in the paper, but it may be worth while referring to some of them merely to avoid giving undue prominence to the points of divergence.

Mr Priest agrees with my views on the functions of a so-called "white" light in colorimetrics. He also suggests the desirability of employing distinctive names for the particular stimuli chosen to fulfil these two logically separate functions. He also agrees that for a large proportion of colorimetric work, in which special conditions do not dictate otherwise, it is desirable on practical grounds to combine both of these functions in one illuminant.

I entirely agree with the suggestion that distinctive names should be given to those things, though I don't quite like the actual terms suggested. This again is largely a question of one's way of regarding matters, but from my point of view it is wrong to refer to the radiation incident on a specimen as a stimulus. It is only radiation incident on the retina which is a stimulus, that is to say, the radiation which leaves the specimen and not that which is incident on it. The incident radiation must, I think, be referred to as the *Illumination*, with a suitable adjective to indicate that a particular quality of illumination has the special significance intended by Mr Priest's term *prime stimulus*. We might refer to it as the *prime illumination*, but I fear that "prime" as a word is too similar to "primary" and is apt, therefore, to have its significance confused in the minds of some of those who would be required to use it. In any case we require more than one standard illuminant in colorimetry. At present two are called for, one approximately white in colour, for the general colorimetry of materials, and the yellower illuminant (colour temperature 2360° K.) which is used in the specification of signal glasses. Future technical developments in applied colorimetrics may show the desirability, envisaged by Mr Priest, of establishing several standard qualities of illumination. The use of a nomenclature or symbolism which will differentiate between these without any possibility of confusion is highly desirable, and I suggest that the illumination provided by any source which may be adopted as a colorimetric standard would be adequately and unambiguously denoted by the term *standard illumination* (T), where T is the colour-temperature of the illumination. It would be understood that this term did not refer to the energy distribution of a black body at $T°$ K., or to any indiscriminately chosen light source giving this colour temperature, but referred to the illumination provided by the particular source agreed upon as the standard illuminant, of about this colour-temperature, for colorimetric purposes. Thus *standard illumination* (2360) would not mean Planckian radiation corresponding to this temperature, nor would it mean illumination from an acetylene flame. It would mean radiation from a vacuum tungsten lamp operated at a colour-temperature of 2360° K., this being

the actual type of illuminant employed in the colorimetric specification of signal glasses at the present time. Similarly, if the present National Physical Laboratory "standard white" were adopted as standard for general colorimetry, the illumination provided by this source and filter combination could be designated *standard illumination* (4800), and the term would be understood to refer not to any illumination which might have this colour-temperature, but only to the quality of illumination provided by the standard source. Any other standard which may be chosen in place of, or in addition to, the present N.P.L. standard could be similarly designated by its approximate colour-temperature appended to the term *standard illumination*. Obviously in contexts where only one standard illumination could be concerned the colour-temperature could be omitted.

With regard to the designation of a standard stimulus to occupy the centre of the colour triangle, that is, to be given the arbitrary scale values $r = g = b = 1/3$, I think Mr Priest's suggestion of the term *basic stimulus*, taken by itself, is excellent. It appears to me, however, that in revising our nomenclature on these matters we should endeavour to employ a common terminology for those things which are identical in function. As will be evident from my paper, I regard the three so-called primaries and the central white of a trichromatic system simply as the four fixed points of a metrological system. For this reason I have become accustomed in conversation on these matters to speak not of three primaries and white but of the four primary stimuli of the system. The identification of the individual members is easily accomplished by appending the general colour names which are most appropriate to them, as, for instance, by referring to the red primary, the green primary, the blue primary, *and the white primary*; white, in the last case, being used in the same general descriptive sense as red, green or blue in the case of the others.

My objection to the term *basic stimulus* as suggested by Mr Priest is that the three other primaries are in exactly the same sense basic stimuli of the system, and to apply the name to one of them alone is analogous to using a special name for one leg of a table.

My objection would disappear if it were proposed to apply the term to all four, replacing the current use of the term primary altogether. This term has now a retarding influence on the spread of correct conceptions on colour measurement as, in the minds of those who are possessed of that little knowledge which is proverbially a dangerous thing, the word is inextricably associated with ideas about the so-called primary colours, or with the primary sensations of the trichromatic theory of vision. For that reason it would unquestionably be an advantage to cease using *primary* in connexion with stimulus relations and leave the word to be applied in discussions of visual theory in which it may have its real significance. If Mr Priest would be willing to extend the use of his term *basic stimulus* to include all four, differentiating between them when necessary by means of the appropriate colour names, I would personally welcome its adoption as a marked advance in the simplification and clarification of colorimetric nomenclature.

With regard to the use of a term to denote a quality of illumination which serves the double purpose of being the prime stimulus and also the basic stimulus in

Mr Priest's suggested terminology, or of being the standard illumination and also the white basic stimulus in the alternative terminology suggested above, I am not sure that a term of this kind would be useful or that it might not, in fact, tend to increase confusion of thought. It is merely a matter of practical convenience to obtain these two things from the same light-source, and it is essential to clear thinking that they should be kept quite separate in the mind of the colorimetrist and in the nomenclature he uses. The white basic stimulus is an essential part of the metrological system; the standard illumination is not. Metrologically there is no significance in the accident that it may be practically convenient to illuminate specimens with the same illuminant as is used to provide one of the fixed points of the measuring system; and the use of a joint term embracing both of these functions, while it might prove convenient in some connexions, is almost certain, in my experience of the effects of words on people's ideas, to have the serious disadvantage of suggesting an inherent association of two quite distinct and unconnected functions.

As Mr Priest's suggested terminology and my own modified proposals have only been introduced here as matters for discussion, I will revert, for the remainder of this contribution, to the terminology used in the body of the paper, in which the terms *primary* and *standard white* have the significances therein attributed to them.

The outstanding difference of a practical character between Mr Priest's views and mine relate to the best colour of the standard white, for a system intended to meet commercial and technical requirements. While this matter is not strictly relevant to the paper, which deals with the principles on which a system should be built, and only introduces the present National Physical Laboratory standards as an illustration of one system which fulfils the principles, the matter is nevertheless of such importance as to justify its discussion at some length.

I regret that in emphasising the importance of fundamental metrological considerations I may have unwittingly conveyed the impression that I attach no importance to other considerations. Metrological considerations, *and those alone*, must determine the *nature* of the standards and the basis of their specification, but other considerations of a practical character must be called in to determine the actual *choice* of standards. It will be found in my paper that it is precisely on such practical considerations that I base the choice of the particular fixed points adopted in the N.P.L. system. With regard to the white I think, as a matter of fact, that the considerations given in the paper are identical in general character with those put forward by Mr Priest, that is to say, considerations designed to arrive at the best compromise between the various requirements of such a source, taking into account the predilections of the technical colourist who has been accustomed to do his colour matching by daylight. The difference between us is that our consideration on such points has led us to different conclusions. Mr Priest feels that it is unreasonable to expect the practical colourist to accept an illuminant yellower than average daylight, which he regards as well represented by a colour-temperature of 6500° K. That is certainly not our experience in this country, where the N.P.L. standard white has been brought into use in the two principal research laboratories of the colour industries without difficulty and, as far as I am aware, with complete satisfaction as regards its colour.

The choice of north skylight or an overcast sky for colour matching has been made in the past by the practical colourist not because of any special suitability of colour, but for the sake of reasonable uniformity. The real technical expert in the industrial laboratories is not, in my experience, wedded to any particular kind of "white" light, and will gladly use whatever is prescribed for him provided its colour is within reasonable limits. The range covered by the phrase "reasonable limits" is not a question of principle and cannot be decided by *a priori* arguments but only by the findings of those concerned with the practical solution of industrial problems. In obtaining those findings, it is, however, necessary to consult the technical experts in those industries, and to avoid attaching undue weight to the so-called "practical man," of the works foreman type, who still lingers on in many of our industries as a relic of the days when a spoonful of rabbit's blood, or some similar ingredient, was regarded as an essential element in producing a high-grade steel. The evidence of the rabbit's blood type of expert should always be taken in conjunction with that other culinary delicacy, a pinch of salt.

It seems certain, as far as my information goes, that the comparatively low-temperature white provided by the N.P.L. standard illuminant is within the range acceptable to the technical experts in the British colour industries. The higher temperature proposed by Mr Priest might, of course, prove equally acceptable as regards colour. But unless it were markedly preferable in that respect, and not merely equally acceptable, there are other important practical objections attached to the bluer source which render it less convenient.

The bluer source requires a more concentrated filter, which has the effects of:

(*a*) rendering its constancy more dependent on the purity of the ingredients from which it is made up;

(*b*) increasing its temperature coefficient;

(*c*) increasing the "lumps and hollows" in its energy distribution curve;

(*d*) reducing its transmission.

These are not mere debating points. Every one of them is of real importance in obtaining the best compromise standard for practical colorimetry. The general desirability of keeping the difficulties introduced by (*a*), (*b*) and (*c*) above, as low as possible, are obvious. The importance of (*d*) is more likely to be overlooked under the impression that it is always possible to get plenty of light by bringing the lamp a little nearer the sample. This is true enough for the great majority of colours, but not for those of low reflectivity, such as dark browns, navy blues, etc., all of which are as important as the brighter colours to the industrial colorimetrist. At the N.P.L., and in those laboratories which adopt our technique, we employ as illuminant a 1000-watt projection type gas-filled lamp on a three-metre bench, the white-light filter being mounted just in front of the specimen. With this arrangement measurements of high observational accuracy can be made on very dark specimens by bringing the lamp near enough to obtain a good working field brightness. But with colours such as navy blue, dark brown, "black," etc., great care has to be taken to prevent serious heating of the filter and consequent alteration of its properties (to say nothing about risk of collapse of the cell), and special care is also required in computing brightness factors from the inverse square law owing to the nearness of the lamp

to the specimen. Anything which would increase such difficulties (which are not merely theoretical but real difficulties to be encountered in some types of work) could only be justified if there were a vital reason for regarding a bluer white than the N.P.L. standard as *necessary*, and not merely as preferable from the point of view of conformity with the rough-and-ready tradition of the pre-standardisation days.

Unless I am placing a wrong interpretation on some of Mr Priest's suggestions there appears to be one point on which we do differ, not simply on a question of practical compromise, as in the matter of the most suitable white light, but on something fundamental. Strictly speaking it is not on any question of colorimetrics, but on the fundamental theory of standardisation. I understand him to envisage two standards for the white light; one an ideal stimulus or illumination, defined by its energy distribution; the other a working standard stimulus, or illumination, provided by a source designed to give the same colour and approximately the same energy distribution as the ideal, and defined by its own actual energy distribution. There are really two points involved here; one relates to the legitimate nature of a standard, and the other to the essential feature of a specification.

There can only be two conceivable reasons for introducing an ideal standard in addition to a working standard. Either it is intended that the ideal standard shall be *used* in theoretical computations instead of the working standard, or else its function is merely to give to the standard system a halo of dignity which an actual working standard is for some reason supposed not to possess. If its use in computational work is intended this cuts across one of the chief functions of standardisation, which is to bring closer and closer the results of all legitimate methods, either direct or derived. Where sub-standards are used in any branch of metrology they must be related to the standard in such a way that the more accurately they are used the less will be the discrepancy between measurements made by their use and measurements made by the use of (or computed from) the standard. In other words the sub-standards must be capable of defining the *same* system as the standard. If the type of thing to be measured is such that the standard requires n independent variables for a description of its essential properties, anything used as a sub-standard must be independently adjustable in each of these n variables.

Now in the case under consideration the standard requires an infinite number* of variables for its adequate description. No sub-standards in the usual sense of the term are possible. A working standard illumination obtained from any practicable source cannot stand with respect to any ideal illumination in the relation of sub-standard to standard. They necessarily constitute two separate standards, and one or other is redundant and undesirable. Clearly the one which must be retained is that which can be used for all purposes, experimental or computational. The energy distribution pertaining to a practicable working standard illuminant must be used for computation of colour from spectrophotometric data, and is the only relevant standard in any complete system of colorimetry.

The second point relates to the specification of the standard. I have dealt in the paper with the vital distinction between the *specification* of a standard and the

* In practice a finite but large number.

description of its essential properties. You cannot legitimately *specify* anything by a description of properties other than those which are independently adjustable to the values stated in the specification.

It may be asked: what does it matter in practice? Why strain at a gnat when we are at present obliged to swallow various camels in the shape of defective technique and defective experimental data?

If need be a small book could be written on the possibility of discrepancies arising from a faulty *basis* of standardisation which would matter seriously in various types of investigation, but fortunately (for the reader as well as the writer) this is not necessary. The whole purpose of the general laws underlying any science is to save people the trouble of writing (or reading) books devoted to the *ad hoc* discussion of particular cases. It is sufficient to show that a procedure violates a basic precept of metrology to render its adoption unjustifiable. It is unnecessary to adduce proof of the extent of the calamity which may come on us if we neglect the precept. Why trouble to neglect it when by observing it we get everything we require and at the same time avoid the risks? I am supposing, of course, that the precept in question *is* a generally accepted one. The particular precept which I am advancing in the present connexion is fundamentally imbedded in the philosophy of modern physics. In relation to things to be produced or set up from specification (and all standards are things of this type) we must define the essentially independent elements of the *procedure* by which they are to be produced, and not the *characteristics* which we hope, desire, or expect them to have when produced. This procedure may be of no interest to us when once we have set up the standard, but that is beside the point. Once we have the standard we can find out experimentally as many of its properties as are required to *describe* it completely for our purposes; but we must set it up first, and that is the necessary and sufficient purpose of the specification. This view is in no respect a special fad of the writer's; it is inherent in the theory of measurement. Any system of standards in which it is neglected (and unfortunately there are some in existence) inevitably must break down in some of its legitimate applications.

Thus, in our present problem, our standard illuminant must be specified by the procedure for setting it up. Once it is specified in this way we must accumulate as much accurate information as possible about its actual properties, and such information must be the basis of all computational work connected with standard colorimetry.

It is possible that in stressing the foregoing points I have read something into Mr Priest's words which is not intended to be there. In any case the fact that I have devoted so much space to the matters on which we differ (or on which I suppose us to differ) should not be allowed to overshadow the many important points on which we are agreed. It is the points of difference, not the points of agreement, that require discussion, so they naturally occupy the space; but the points of agreement are not less important on that account, and I wish to conclude as I began by thanking Mr Priest for an interesting and valuable note, and by expressing the conviction that it brings definitely nearer the possibility of a useful measure of international agreement on these matters.

In reply to Mr Wright's contribution it would appear for some reason that he and I differ more on paper than in our verbal discussions of these matters. Some of the points raised in his written communication are dealt with in my reply to Mr Priest's communication. I cannot agree that the energy curve specification could be made a practicable specification by inserting tolerances. Tolerances have no place in the specification of a *standard* of any kind. Tolerances enter into the instructions or contracts prepared for the guidance of those who are endeavouring to make something or do something in accordance with a standard. If I want a piece of material a metre in length I may specify a tolerance of a centimetre, a millimetre or a ten thousandth of a millimetre depending on the object I have in view and the accuracy it necessitates, but it would be absurd to have a tolerance in the specification of the standard metre.

With regard to choice of primaries I have dealt with this matter at length in the paper and need not repeat the arguments here. Mr Wright appears to envisage that in addition to a metrologically sound system of general applicability, a modified system might also be evolved for the limited but important purposes of commercial standardisation. My feeling on this matter is that any advantages accruing from this procedure would be very much outweighed by the enormous disadvantage resulting from the effective divorcement of the scientific and industrial investigator, and the trouble which each would be put to in translating the other man's language when their fields of interest overlap. This difficulty, which would naturally be most felt by the industrial colorimetrist, would inevitably result in loss of touch with general scientific developments and the ultimate confinement of technical developments to grooves cut out by the industrial workers themselves as in the bad old past from which they are at present laudably struggling to get away. I think it will be conceded that the closeness of touch between industry and science which is desirable in the colour industries as in all others, is only possible if the same language and same metrical system is used by both. If this is so it is clearly the scientist's system which must be used, and not a system limited in its utility to industrial needs and inadequate for the other legitimate purposes to which the scientist may have occasion to put it.

As regards the future of commercial colorimetry it is too early to say what methods will ultimately be found of most use. I do not however share the optimism of Mr Wright with regard to the spectrophotometric method. There are numerous cases of practical interest to the industrial colorimetrist in which it is, for practical reasons, quite impossible to obtain spectrophotometric data of anything like adequate accuracy for colour specification by any spectrophotometric equipment yet described. Apart from this the time element is a serious disadvantage of the spectrophotometric method as a basis for routine commercial colorimetry. There has already been carried out in the industrial research laboratories of this country a large volume of important investigatory work on colorimetric properties of materials which would have been quite impracticable, both for technical and economic reasons, except by direct colorimetry.

ON ABSOLUTE REFRACTIVE INDICES IN GEOMETRICAL OPTICS

By T. SMITH, M.A., F.Inst.P.

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MS. received, 17th February, 1931. Read, 12th March, 1931.

ABSTRACT. Expressions are found for the computation of a system of fundamental coefficients for optical instruments which involve relative refractive indices only. The system is less convenient for numerical work than those now in use. The convention which assigns absolute values to the indices of materials is thus distinctly useful though of no physical significance.

When a ray is traced through a refracting surface the direction of the emergent ray depends on the refractive index of the second medium relative to that of the first. The absolute values of the indices, which are defined by means of an arbitrary convention, are not significant. Similarly the properties of any optical instrument depend on relative, not on absolute, refractive indices. On the other hand the quantities by which we are in the habit of describing these properties are not independent of the absolute values. Thus the power of any system is a homogeneous function of the first order in the absolute indices. The separations of the various surfaces are also always associated with the inverse of the refractive index. Alternatively if we specify the positions of the surfaces by the separations of their centres of curvature and therefore replace powers by modified powers, the separations enter into the formulae multiplied by a refractive index, and the modified powers involve indices to the power -1. The convention thus affects the scales we use for measuring the properties of optical systems but is not connected with the properties themselves.

It is not unnatural to enquire whether a good reason exists for dragging in this convention when the properties in which we are interested in no way depend on it. We should expect to find that the quantities which we are in the habit of using can be replaced by others in which relative indices alone appear. The separations between lenses and distances along rays would almost inevitably then appear in their natural measure, so that an inch measured along a ray in glass would be of exactly the same value as an inch in air. Before we can come to a useful decision on this point we must determine the nature of the system which has these properties.

It will only be necessary for us to consider a simple case, and we will take the secondary equation for refraction at a single surface

$$\frac{\mu}{u}+\frac{\mu'}{v}=a=(\mu'\cos\phi'-\mu\cos\phi)\frac{1}{r} \qquad \text{.....(1),}$$

where a is the power and u and v are the distances of conjugate object and image

points from the point of refraction. These distances are positive if the object and image are real. It is clear that we can only modify the ratio of the coefficients of $\frac{1}{u}$ and $\frac{1}{v}$ by changes of origin. In order to retain a simple form the new origins must be conjugate points. Putting $u = u' + \epsilon$, $v = v' + \eta$, where

$$\frac{\mu}{\epsilon} + \frac{\mu'}{\eta} = a \qquad \text{.....(2),}$$

we obtain

$$\frac{\mu - a\epsilon}{u'} + \frac{\mu' - a\eta}{v'} = a \qquad \text{.....(3).}$$

The coefficients of u' and v' are equal, and (2) is also satisfied, if

$$\mu - a\epsilon = \mu' - a\eta = (\mu\mu')^{\frac{1}{2}} \qquad \text{.....(4).}$$

The equation can now be made of zero dimensions in the refractive index by dividing throughout by this quantity. The modified equation is therefore

$$\frac{1}{u'} + \frac{1}{v'} = \alpha = \left(m\cos\phi' - \frac{1}{m}\cos\phi\right)\frac{1}{r} \qquad \text{.....(5),}$$

where m is the square root of the relative refractive index μ'/μ. The new power α always differs from the old value a.

When refraction takes place through a series of surfaces the general equation for conjugate points will take the form

$$(-1 \quad u')\begin{pmatrix}\beta & \delta\\ \alpha & \gamma\end{pmatrix}\begin{pmatrix}v'\\ -1\end{pmatrix} = 0,$$

where

$$\begin{pmatrix}\beta & \delta\\ \alpha & \gamma\end{pmatrix} = \begin{pmatrix}1 & .\\ a_1 & 1\end{pmatrix}\begin{pmatrix}1 & -\tau_1\\ . & 1\end{pmatrix}\begin{pmatrix}1 & .\\ a_2 & 1\end{pmatrix}\begin{pmatrix}1 & -\tau_2\\ . & 1\end{pmatrix}\ldots,$$

$a_1, a_2, \ldots$ being the powers of the surfaces according to the new definition, and $\tau_1, \tau_2, \ldots$ the separations of successive reference origins along the ray. It is thus possible to satisfy the conditions we have had under consideration.

In theory the new equations are as acceptable as the old, but for numerical work their weakness is obvious. The distances τ are not measured between points which are given in the specification of the system nor are they derivable without special computation. By equation (4) we see that

$$\epsilon = \left(\frac{1}{m} - 1\right)\frac{1}{\alpha}, \quad \eta = (m-1)\frac{1}{\alpha},$$

and the origins thus vary with the relative index at the corresponding refraction, i.e. they vary with the wave-length of the light. It follows that no saving of labour in computation can be effected by the adoption of this system. The system obtained by merely dividing equation (1) throughout by $(\mu\mu')^{\frac{1}{2}}$ would be distinctly clumsy.

Our conclusion is therefore that the convention by which absolute values are assigned to refractive indices is a distinct aid in the computations required in geometrical optics, though it is without meaning in the use made of the quantities so found. In other words, the convention is valuable and of no significance.

OBITUARY NOTICE

COLONEL J. W. GIFFORD, F.R.A.S.

THE death on 27th October, 1930, at the age of 74, of Colonel J. W. Gifford takes away a Fellow who was better known to most of us by his work than in his person. His optical papers were published after he had reached the age of 45; and his business and numerous local activities, and in the last few years his failing health, left him little opportunity for being present at our meetings.

Members of the Optical Society are familiar with the series of papers communicated by Colonel Gifford to the Society from 1914 to 1929 in connection with the computation of lenses, and the other papers on the same subject communicated to the *Monthly Notices* of the Royal Astronomical Society from 1908 to 1924. In these papers, and in the book *Lens Computing*, in which he described the methods used by him, evidence is given of the meticulous care which Colonel Gifford brought to bear on his scientific work, whether of observation or calculation.

Probably the most important of his optical work, however, is embodied in a series of seven papers communicated to the Royal Society and published in the *Proceedings* from 1902 to 1922. These papers dealt with the refractive indices (and temperature variations of refractive indices) of fluorite, quartz, Iceland Spar, vitreous silica, sea water, fresh water, and a number of optical glasses. Colonel Gifford's measurements of refractive indices are quoted in all authoritative collections of optical data, and have doubtless been used in optical calculations all over the world.

Further Royal Society papers in collaboration with Prof. T. M. Lowry on the refractive indices of benzene, cyclohexane, and nicotine, are of interest to the organic chemist.

What is not so widely known to members of this Society is the very exceptional range of Colonel Gifford's other activities. From early manhood he was an active member of the firm of Gifford, Fox and Co., in his native town of Chard, Somerset, of which firm he was still Chairman of the Board of Directors at the time of his death. In that capacity he was not only intimately concerned with the commercial and manufacturing side of the business, but was in personal touch with his employees, and an organiser of schemes for their benefit.

He was also prominent in the affairs of the town of Chard and foremost in every activity for its welfare. To give one instance only, Colonel Gifford maintained for thirty years a nurse for the town and district of Chard entirely at his own expense, supplementing this with gifts of money for necessitous cases. He was elected a member of the Town Council and endeavours were made to obtain his consent to nomination as Mayor, but unsuccessfully.

A third group of activities consisted in the performance of the duties of a landed proprietor, duties which, though rather thankless in recent years, have played so great a part in the stability and development of England.

These engrossing affairs would have filled the life of most men, but not that of Colonel Gifford. Joining the old volunteer force in 1882, he subsequently commanded the 5th Somersets, and was appointed Honorary Colonel of the Battalion, throwing into his work for the volunteers and, subsequently, the territorials, the energy and thoroughness which he brought to everything he did. It was, doubtless, this interest in military affairs that made him realise the importance of telescopes in the great war, and was the reason of his having constructed, at his own expense and from his own calculations and materials, many hundreds of telescopes for the troops which, owing to the very high standard which he demanded from himself in calculation and from the maker in manufacture, were in all probability better than anything else of the kind available.

So much for his life as employer, citizen, country gentleman, and soldier. We come now to the occupations of his leisure. He is stated to have been one of the two first men in this country to take and show an X-ray photograph, both exhibiting their photographs in London on the same day in 1896; and he frequently did X-ray work for the Chard doctors from then up to within a few days of his death.

Microscopy was another hobby, and here again he was always ready to place his microscope and his skill in its manipulation at the service of local medical men. He was also interested very early in the discovery of radium; at a time when very little was available for hospital work he gave an important quantity to the Middlesex Hospital.

He played both the violin and piano, and among outdoor sports tennis; spoke French, German, and Italian, read Greek and Latin easily, and knew a little modern Greek, Hebrew, and Chaldee. Yet of all his occupations and recreations none—at any rate in the latter part of his life—seemed to give him greater happiness than the careful measurement and utilisation in calculation of refractive indices of optical materials. The goniometers which he had specially built for his measurements have been bequeathed by him to the Imperial College.

Colonel Gifford married in 1883 Emma, daughter of the late Mr Ernest Rossiter, solicitor, of Taunton, and had one son and four daughters, all of whom survive him. No notice of Colonel Gifford's scientific work would be complete without mention of that of his wife, who has been at hand with sympathy, enthusiasm, and help in this as in all his other activities.

Colonel Gifford's work in geometrical optics prompted and stimulated the preparation by Mrs Gifford of a book of eight-figure tables of Natural Sines tabulated in seconds, calculated by interpolation from the *Opus Palatinum* of Rheticus. This volume, representing a very considerable amount of labour, appeared in 1914 and is now in its second edition. Being the only work of its class it has been found of considerable value to computers in optics and other branches of science. Mrs Gifford followed it in 1927 by a parallel volume of Natural Tangents of the same extensive proportions and scope. These two works have supplied a very decided impetus not only to the development of scientific applications but also to the introduction and general recognition of the modern methods of calculation by machine.

F. T.

REVIEWS

The Selenium Cell, its Properties and Applications. By G. B. BARNARD. Pp. xxix + 331; 258 Figs. (London: Constable and Co., Ltd., 1930.) Price 35*s.* net.

It is not until an account is collected within the pages of a single volume that one realises something of the immensity and complexity of the mass of literature that has grown round the subject of selenium; indeed, so great in quantity and so ill co-ordinated was the work spent on this fascinating element that there existed, as there exists in other regions, considerable danger of uselessly overlapping researches, a danger which has only now been removed, or minimised, by the timely appearance of Mr Barnard's most comprehensive work.

A comparison with a companion volume on photo-electric cells serves to emphasise the remarkable difference in the history of these two devices, so similar in their effects. From the year of the discovery of the effect of light on selenium, interest in its applications has never flagged, and the passage of fifty-seven years sees the output of literature and of work on the subject greater possibly than at any time in its previous history. The theories of the action of light on selenium, many though they be (and they are admirably reviewed by Mr Barnard) have proved secondary in interest to the applications of the phenomenon —a curious reversal of the state of matters which applies to the companion device, the photo-electric cell.

Mr Barnard's treatment of the subject is divided into two parts. A discussion of the properties of selenium and of selenium cells occupies some 180 pages, and includes a detailed account of the construction of the different types of selenium cell, and a summary of the theories of the action of light on selenium. The second part is concerned with the practical applications of selenium cells. This very wide subject is grouped conveniently into four chapters. One chapter deals with miscellaneous applications of the fire alarm, fog indicator, automatically controlled lights, and gramophone pick-up type, a second chapter with photometric measurements, a third with the optophone and with light telephony, and a fourth with applications to talking films, telephotography and television. It is in these chapters that Mr Barnard's flair for exposition shows itself most favourably; his explanations are always clear and are models of condensation.

A number of appendices give in very full detail the physical constants of selenium. The bibliography is, as regards number of references, a little difficult to assess, but in the aggregate must amount to close on a thousand. Of its value there can be no question.

The volume can be strongly commended, alike as a general treatise and as a full and comprehensive book of reference.

Photo-electric Cells and their Applications. A Discussion at a Joint Meeting of the Physical and Optical Societies, June 4 and 5, 1930. Pp. 236; 107 Figs. (London: The Physical and Optical Societies, 1930.) Price 12*s.* 6*d.* net.

This publication, which consists of a collection of papers read and discussed at a joint meeting of the Physical and Optical Societies, contains the results of the most recent work in this branch of physics. Introduced by Prof. H. S. Allen with a paper dealing briefly with the history and laws of photo-electricity, there follows a sequence of papers of great variety, all dealing with different aspects of the same subject.

In these days of talkies and television, the light-sensitive cell has obtained an unexpected publicity, and it is interesting and pleasing to find the selenium cell making a bid for its

share of attention against the younger, but more modern, photo-electric cell. Indeed, the selenium cell still holds its own in certain spheres where great sensitivity without too much valve amplification is required, and Prof. H. Thirring has contributed a paper singing its praises, even for use in sound reproduction where one would expect its characteristic sluggishness to be fatal.

The introduction is followed by two papers by Campbell, one on the "Theory of Photo-Electric Emission" and the other on the "Standardisation of Photo-Electric Cells," a very much needed standard. He bases his suggested standards on the requirements necessary for sound reproduction, both as regards shape and sensitivity. It is certainly time some convention of this nature was adopted. This brings us to the paper by Davies and Ruff indicating the suitability of the thin-film caesium cell for sound reproduction. These cells are far more red-sensitive than the ordinary potassium or sodium cell, and are for this reason more suitable for use with the activating light used, the maximum emission of which is well in the red.

"The Time-Lag in Gas-Filled Photo-Electric Cells," by Campbell and Stoodley, offers an interesting comparison with the paper by Prof. Thirring, which deals with the time-lag in the selenium cell. One must admit the superiority of the photo-electric cell in this respect, but Prof. Thirring makes a very good case.

Two of the most ingenious papers are contributed by Dobson and Perfect, and Dobson, on a method of comparing very small amounts of light by means of a photo-electric cell and valve amplifier, and "A Spectrophotometer for Measuring the amount of Ozone in the Upper Atmosphere." The authors of these papers have carried the use of the photo-electric cell to a fine art. Another paper by Perfect on "A Method of Measuring Small Angles" must be included in the same class, and is a good example of the application of a light-sensitive cell for an ultimate measurement of a quantity quite different from light.

Amongst other papers worthy of special mention is that by Geffcken and Richter which deals with a new "screened cell" constructed somewhat after the manner of the screened grid valve. Harrison deals with the photo-electric cell as a precision instrument in photometry, he points out the difficulties and remedies, and has used the cell to great advantage. Cells sensitive to infra-red radiation are discussed—in particular "The 'Thalofide' Cell" by Case is worthy of mention—"Thalofide" being derived from the words "thallium sulphide."

Two papers must be mentioned which remind us that the application of photo-electric cells is really extensive—"The Use of Photo-Electric Cells in Biological Work," and "Photo-Electric Cells in Relation to Photo-Therapy." This must suffice, for it is impossible to mention all the thirty-one papers individually, though they are all worthy of mention.

It may be said in conclusion that this collection of papers is more interesting than a book for this reason: in a book the subject-matter in the chapters follows in a logical sequence, so that one has a good idea what will come next, whereas in this collection one is transported from one thing to another, as one passes from paper to paper, and "variety is the spice of life" they say! G. B. H.

The Theory and Design of Illuminating Engineering Equipment. By L. B. W. JOLLEY, M.A., M.I.E.E., R. M. WALDRAM, B.Sc., and G. H. WILSON, B.Sc., A.M.I.E.E. Pp. xxxi + 710; 571 Figs. (London: Chapman and Hall, 1930.) Price 45*s.* net.

There are among us some who, seeing the rising tide of scientific literature which pours in "handbooks" of ever-increasing portentousness from the German press, are wont to speak with a measure of bitterness of the lack of enterprise shown by British publishers. We are righteously confounded by the appearance of a massive volume such as the one before us—made up of some seven hundred ten-by-six inch pages and weighing close on

five pounds—which deals, not, be it noted, with illumination in general, not even with illuminating engineering, but with the more restricted subject of illuminating engineering equipment.

Restricted as the title is, the authors have found it necessary to give a certain amount of preliminary information, for the sake, apparently, of making the volume complete in itself. Thus Part I, which deals with fundamental principles and occupies some fifty pages, gives a concise and very clear account of the methods of calculation commonly employed and the physiological principles involved in illumination problems. In what is professedly a brief account, the selection of matter is necessarily left to the discretion of the author, but, inasmuch as the Weber-Fechner ratio is briefly discussed, it would seem that the insertion of a passing reference to Houstoun's recent work would add much to the interest of this section. Houstoun's method of plotting $I/\Delta I$ against log I—quite apart from his observation that the points so plotted lie on a Gaussian curve of errors—marks a distinct advance. For if we remember that ΔI is really a differential coefficient which should be written $\Delta I/\Delta P$, where ΔP is an element of perception, we see that the elementary area ydx under the curve obtained represents the element ΔP; and hence the total area included under the curve, the x-axis and any ordinate represents the total perception up to the intensity represented by that ordinate—that is, if elements of perception *can* be added. That, however, is another matter.

The book is divided into five parts. Part I, as we have said, deals with fundamental principles. Part II is concerned with light sources and, after an introductory section dealing with the physics of light production, takes up in turn the consideration of incandescent lamps, arc lamps, and gaseous conduction lamps. Part III, which is by far the largest part of the book, is written under the heading "light equipment" and treats in very considerable detail of such topics as types of lighting equipment and their mechanical design, reflecting, refracting, diffusing, and colour media, optical projecting systems, and of such special problems as are met with in stage, studio, and airport lighting, and in the lighting of advertisement signs.

Part IV deals with the auxiliary equipment necessary for street lighting, traffic control, theatre and studio equipment, advertisement sign flashes, remote and automatic control, and automobile lighting auxiliaries.

Not the least valuable part of the book is Part V, where, under the heading of Appendices, is grouped a number of Tables of various data which occupy some fifty pages.

Obviously, the book covers a vast range; even so, it is, as is remarked in the preface, far from being complete, certain specialised forms of equipment, such as those for microscope illumination, being omitted. But as it stands the authors have assembled, in an orderly and competent fashion, representing an almost incredible amount of patient and critical work, more detailed information concerning the subject of illumination equipment than has ever before been collected between the boards of a single volume.

Where so much has been given, it may seem ungrateful to ask for more; but the present reviewer would wish to see certain parts of the book developed in much greater detail; drifting signs and flashers form cases in point where the information given is too little for those whose knowledge of the subject is scanty, and is unnecessary for the expert. This added information could possibly be obtained, without increasing unduly the size of an already large volume, by cutting down those pages which deal with the general principles of illumination, the physics of light production, and the elements of geometrical optics—with such topics, in short, as may be found in more ordinary and more easily accessible volumes.

The volume is unique; it is indispensable to the student of illuminating engineering, and the reviewer looks forward with interest to the appearance of a second edition which shall cast its net even more widely than does this first edition.

A. F.

Annual Report of the Smithsonian Institution, 1928. Pp. xii + 763; 49 Figs. and 145 Plates. (Washington: U.S. Government Printing Office, 1929.) Price $2.00.

The *Annual Report of the Smithsonian Institution* is something of a lucky dip. The routine work of the Institution is well known and we need concern ourselves no further with it than to note that appendix eight of the secretary's report deals with the future prospects of the international catalogue of scientific literature, and that appendix seven gives a résumé of the work of the Astrophysical Observatory. The writer of the appendix last named gives a brief but intriguing account of radiometer measurements of the distribution of energy in the spectra of the stars, for which radiometers almost of microscopic size were constructed, the vanes being made of portions of the wings of house-flies. (Those who are thrilled by a knowledge of the number of postcards required to cover the surface of the largest English county will be happy to learn that it requires about six million house-flies' wings to weigh a pound.)

For the general reader by far the most interesting part of the Report is the general appendix, which consists of "memoirs illustrating the more remarkable and important developments in physical and biological discovery." The terms of reference are wide, the interpretation thereof is generous; and he who will may read at leisure of the wider aspects of cosmogony, island galaxies, astronomical telescopes, cosmic rays, continental displacements, mammalogy (*horresco referens*), water divining, the ecology of the red squirrel, and the physiology of the ductless glands, to cite but a few of the titles of some twenty-seven articles.

The essays are popular without being cheap, are eminently readable, and are written by authorities in the particular branches of science with which they are concerned. Not the least interesting are the appreciation of Arrhenius by Sir James Walker, and that of Theodore Richards by G. P. Baxter.

A. F.

ERRATA

In the paper entitled "The Logarithmic Wedge Sector and its Use in Quantitative Spectrum Analysis" by F. TWYMAN and F. SIMEON (Vol. **31**, No. 4):

p. 169, equation (i) *should read* $-\log\theta = 0{\cdot}3 + 0{\cdot}2l$.

p. 171, equation (iii) *should read* $\log\dfrac{I_A}{I_B} = 0{\cdot}2\,p\,(l_A - l_B)$.

p. 172, equation (iv) *should read* $\log\dfrac{I_{A'}}{I_{B'}} = 0{\cdot}2\,p\,(l_{A'} - l_{B'})$,

hence (iii) and (iv) $\log\dfrac{I_A/I_B}{I_{A'}/I_{B'}} = 0{\cdot}2\,p\,\{(l_A - l_B) - (l_{A'} - l_{B'})\}$.

p. 177, line 11, *for* θ *read* θ_1.